

Psychological Bulletin

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Psychological Bulletin

SOCIAL PSYCHOLOGICAL CORRELATES OF MENTAL ILLNESS AND MENTAL HEALTH¹

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Theories and empirical findings concerning the etiology of mental illness come from a variety of scientific disciplines representing a wide range of levels of analysis, from the biochemical to the historico-cultural. The data and hypotheses are so disparate as to defy incorporation within a single comprehensive theory or research program. It is common practice for each researcher to work, relatively independently of others, within the scope of variables encompassed by his particular scientific discipline. This inevitably restricts the possibilities for interrelating of findings and for interpreting the facts discovered by one researcher within the theoretical framework of another. A further major impediment to the integration of research findings on mental illness and health is the diversity of definitions of these phe-

nomena employed by various researchers (54). In many cases it is not known in what manner mental illness defined by one criterion relates to mental illness defined by another.

Yet from diverse approaches have come hypotheses and facts that conceivably bear relationships to one another, so that an imaginative researcher of the future might well combine them into a more general interpretation of the phenomena of mental illness and health. A summary of some relevant sociological findings has been provided by Clausen (4). The purpose of the present paper is to critically review theories and research from two levels of analysis—the environmental-demographic and the interpersonal—with the aim of suggesting interrelations where possible, or of leaving a confusion of disparate results where necessary. It is possible, in some cases, to interpret findings from the environmental-demographic level of analysis in terms of the next “lower” level, the interpersonal. That is, demographic variables may be presumed effective because of their interpersonal concomitants. By further reduction, it would seem possible to regard the interpersonal factors as operating through the psychological processes of the interacting individuals; and finally one might wish to reinterpret the psychological interpretations by

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reference to "more basic" physiological processes within the organism.

Whether or not one cares to adopt such a reductionist view of the relations among scientific explanations, it is at least apparent that facts at one level of analysis should be potentially coordinatable with facts at another level. Moreover, the researcher working within one system of variables can conceivably be stimulated to search for new facts by a consideration of findings from another domain. The apparent socioeconomic differences in incidence of schizophrenia should thus be of concern to the biochemist; conversely, the relation of nutritional deficiency to pellagra should constitute a relevant fact for the sociologist. The range of variables considered in this report will be much narrower than that spanned by the fields of sociology and biochemistry; yet the possibilities for interrelation should provide at least some stimulation for narrow-gauge, interdisciplinary research.

DEMOGRAPHIC AND ENVIRONMENTAL CORRELATES

External Events

The incidence of mental disorders is sometimes attributed in part to the occurrence of major social events, of either a developing or critical nature. Among the events most frequently considered are increasing technological complexity, war, and depression. There is little direct evidence of the influence of such factors on mental health, since comparable incidence and prevalence data are rarely available over an extended period of time.

Paul (47) has suggested that:

Culture lends purpose and direction to the lives of those it serves. All forms of organization, however, are achieved at some price. The costs may be widely distributed throughout the society in the form of strains built into the typical personality or they can be borne disproportionately by a minority of individ-

uals, those typed as "deviants" by their fellows (47, p. 51).

The implication of this point of view would appear to be that increasing social complexity increases the extent of mental illness, as defined either by hospitalization or by psychological testing. Such an hypothesis has been advanced more explicitly by Leighton (36), to the effect that in areas of rapid technological change mental illness rates should be greater than in other areas.

Indirect, and only partial, evidence relevant to this hypothesis is contained in a study by Goldhamer and Marshall (18) of the records on psychiatric hospitalization during certain periods of the past century in some areas of the Northeastern United States. Evaluation of these data requires complex adjustments for the changing character of the population under study and for changing hospital capacities. The conclusion of the authors is that the incidence of psychoses has not increased in age groups under 50. They note, however, that the rate of industrialization in those areas since 1840 may not have been sufficient to produce an increase, so the question of a possible technological influence on mental health is certainly still open. One can only state that, at this time, there are apparently no systematic empirical data to support the hypothesis.

It has been reported as an empirical fact (10, p. 151) that suicide rates decrease during periods of war, but no direct explanation of this phenomenon is available, except as it may be implied in that offered by Henry and Short (23) in their discussion of suicide and homicide. The latter authors report a negative correlation between suicide rates and the business cycle. The relationships are not simple ones, since they vary for

different demographic groups of the population, but Henry and Short suggest certain unifying sociological principles which may account for a number of diverse facts in this area. These principles will be reported below in the section on interpersonal correlates.

Urban-Rural Differences

Malzberg (41) reports a higher hospital admission rate for New York urban than for rural areas. Rose and Stub (50) find a similar difference for all psychoses except involuntal melancholia, based on state hospital admissions throughout the United States in 1933. The source of this difference is not clear. At least three alternative explanations are plausible: it may be that rural life produces less "cultural strain" in Paul's sense (see preceding quotation) than does urban life; or perhaps there are inherent biological differences in susceptibility bred into urban and rural populations; or it may be that rural groups tend more than urban to care for their psychiatric cases outside of hospitals. Choice among these or other possible explanations is not provided by available empirical data.

At least one exception to this general urban-rural difference is provided by data on suicide rates in Michigan for the period 1945-1949 (53). There suicides among rural males were, at all ages, almost twice as frequent per capita as were suicides among urban males, while female rates were roughly equal for the two populations.

Finally, a review of studies in the United States and Germany (38) indicates that the *ratio* of the incidences of schizophrenia and manic-depressive psychoses is higher in urban than in rural areas. Such evidence on *differential ratios* between admissions rates for different diagnostic categories may be more readily

accepted as reliable than simple rural-urban rate differences, since the question of community tolerance is not so much at issue (see 54). Possible explanations of such findings may be akin to those offered for the urban census tract differences reported below.

Intracity Differences

A number of studies on the ecology of mental disorders in urban areas have been reported, mostly following the pioneering work of Faris and Dunham (12). These rather consistently show hospital admissions rates for schizophrenia to be higher from tracts in the center of the city than from peripheral tracts (6, 7). The manic-depressive psychosis rates (also based on hospital admissions) are not similarly concentrated.

Almost invariably such intracity differences have been attributed to differing socioeconomic characteristics of the tracts or to closely related factors. A more recent study in Syracuse (19) showed hospitalization rates for all psychoses combined, and for senile psychosis and cerebral arteriosclerosis as a single category, to correlate substantially with two socioeconomic characteristics of the census tracts in that city: average monthly rental value (negative relation) and average number of dwelling units per structure (positive relation). Similar correlations with average monthly rental value are reported for Austin (2) and Peoria (52). Draft rejection rates in the Boston area (1941-1942) for mental deficiency, psychopathic personality, and alcoholism were found to correlate negatively with the socioeconomic level of the community (27). The rate of rejection for psychoses also correlated somewhat in the same direction, even though most psychotics presumably did not report for induction physicals.

Faris (10, p. 123) reports that in Chicago hospital admissions for neuroses came, more than for any other disorder, from the highest socioeconomic areas, while alcoholic admissions came from the lowest. The findings regarding neuroses are perhaps interpretable as reflecting greater ability or readiness of wealthier neurotics to obtain psychiatric care. Data from selective service physical examinations in the Boston area showed no appreciable differences among the areas in rates of rejection for neuroses (27).

The ecological studies may be interpreted in at least two different ways: First, more conservatively, as simply demonstrating important facts about the cities studied, namely, the distributions of mental illness within them. Second, more inferentially, as providing evidence regarding etiological factors in mental illness. Dunham himself maintains that their contribution is restricted to the former area (6), but since others have maintained that the studies have causal implications, weaknesses of such an interpretation should be considered.

A major criticism relevant to the first interpretation concerns the criterion for mental illness, namely hospitalization. The possible shortcomings of this criterion have been discussed elsewhere (54). There are, in addition, certain difficulties in establishing an appropriate population base for the admittance rates. To the extent that the lower-status tracts are composed of larger proportions of intercity migrants than the upper-status tracts, figures for any one area would not correctly reflect the actual population base on which the rates are computed. This criticism would not seem to apply to differential migration rates within the city, providing such migration were primarily

horizontal, i.e., among census tracts of similar socioeconomic status. While it seems unlikely that differences in population turnover would account completely for the reported differences in disease incidence, studies employing the necessary corrections have not come to the present reviewer's attention.

The bulk of the criticism has been leveled against etiological interpretations of ecological studies (5, 35, 49). It is generally maintained that there are too many uncontrolled variables to permit assignment of causality to any few selected factors. For example, the higher admittance rates for the lower socioeconomic tracts may indicate that schizophrenics or schizophrenia-disposed persons tend to move into them from elsewhere, rather than that these areas are unusually fertile breeding grounds for the disorder. Data of Schwarz as reported in Clausen & Kohn (5) may be taken as supporting the downward mobility hypothesis: schizophrenics tend to go down in the occupational scale prior to hospitalization. But a recent study in New Haven (24) showed relative stability or, if anything, somewhat disproportionate upward mobility, among schizophrenics. Thus the data are inconclusive in this regard. It is possible that downward mobility occurs more frequently among schizophrenics in Chicago, though not in New Haven.

The general criticism which applies to many ecological studies, whether their results be interpreted in a causal fashion or simply as showing interrelations among variables, is that their data are not directly relevant to many of the hypotheses being tested. Information about average characteristics of census tracts does not necessarily provide information about the particular residents who become ill. If living is more crowded in the high-

incidence tracts, or if social isolation is more prevalent, it might gratuitously be assumed that the patients shared these community characteristics. There are more direct ways of testing such an hypothesis, using correlations based on individual cases, rather than on geographical areas. Such analysis of individual data would perhaps not yield unambiguous causal interpretations, but at least it would demonstrate whether the predominant characteristics of the community reflected characteristics of the mentally ill within them.

Despite this obvious criticism, investigators continue to apply data about communities to hypotheses concerning smaller units of analysis. For example, Leighton (36) hypothesizes that such factors as broken homes, conflicting values, and fear or uncertainty about the future foster psychiatric disorders. Then he indicates an inappropriate unit of analysis by proposing a test of the hypothesis: within a given community, the greater the combination of these stress factors in a given area, the higher should be the illness rates (36, pp. 133, 165).

One should note, however, that in order to regard community characteristics as causative factors in mental illness, it is not necessary to demonstrate that they are shared by persons who become ill. Widespread hostility against minority groups may induce mental illness in such groups, even though the members themselves do not share the hostility. Thus, general social and cultural factors may affect members of a group, whether or not they share them. At this cultural level of analysis, however, it is necessary to regard the communities, rather than individuals, as elements of the population to be studied; statistical tests should be based on degrees of freedom representing the

number of communities studied, rather than the number of residents of these communities.

Socioeconomic Status

When hypotheses regarding social class differences in prevalence or incidence of mental disorder are tested against data on individual cases, the findings corroborate those of the ecological studies. Hospital admissions rates for schizophrenia in New York, Chicago, and New Haven were disproportionately high among members of the lower classes (3, 14, 25, 41, 50). Rates for certain other psychoses, such as alcoholic psychosis, syphilitic psychosis, senile psychosis, and cerebral arteriosclerosis, may show similar patterns (3, 14), but the rates for manic-depressive admissions do not show systematic class differences (3, 41, 50). A review of studies in the United States and Germany (38) indicates that the *ratio* of schizophrenia to manic-depressive psychosis (based on hospital admissions) is higher in the lower than in the upper socioeconomic strata.

Data on rates of psychoneuroses (under treatment) are not consistent (50). Hollingshead and Redlich (25) found larger proportions of the upper classes under treatment for neuroses in their census of the New Haven area hospitals and clinics. While class differences in rates may be partly due to the cost of treatment, this is apparently not the dominant factor, according to the analysis of Myers and Schaffer (44). In an outpatient clinic which charged nominal fees and was open only to people with incomes under \$5,000, the class distribution of persons seen initially by the intake interviewer was approximately like that of the total New Haven population. Yet the class distribution of those who were actually accepted for treatment was concen-

trated more heavily at the upper end of the scale. The higher a person's social class, the more likely he was to be accepted for treatment, treated by highly trained personnel, and treated intensively over a long period. Thus it appears that class differences in rates of neurosis-under-treatment are more a function of social distance between patients and therapist than of economic limitations (25). Specifically, Meyers and Schaffer (44) suggest that the following factors may be relevant to the selection of patients: (a) the psychiatrists' values concerning who should be treated, (b) the potential patients' conceptions of the therapeutic process, and (c) the similarity of potential patients to the therapist with regard to certain traits deemed relevant to success in therapy.

Thus it is not clear to what extent neurosis rates in the various socioeconomic classes are artifacts of the criterion, namely acceptance for treatment, and to what extent they reflect the actual incidence which might be measured by some other criterion such as psychological tests or case finding. Auld (1) reports that most personality tests yield lower scores for lower class subjects than for middle and upper class subjects. While it is not clear to what extent such differences reflect middle class biases of the test-constructors, at least such a discrepancy raises the possibility that the correlation between social class and mental health may be partly a function of the particular criterion chosen.

Even the class differences in prevalence of psychoses reported for New Haven are regarded by the authors as inadequate evidence of true differences in incidence (25, 26). They are conceivably due, at least in part, to differences in the kind of treatment provided for patients in the various classes. Upper and middle

class patients are more likely to receive psychotherapy, while lower class patients are more likely to receive only shock therapy or simply custodial care without treatment. These differences in treatment may produce differences in duration of the illness, thus making for greater discrepancy between prevalence and incidence in the lower classes than in the upper and middle. Hollingshead and Redlich are nevertheless prepared to find class differences in a true incidence study (25) and this expectation appears warranted in the light of social class differences in rates of first admissions to hospitals reported elsewhere (3, 14, 50). (See Kohn [32] for a lucid discussion of prevalence and incidence figures and their use in mental health research.)

The reasons for socioeconomic differences in psychosis rates appear to be inadequately explained at present. Clark (3) suggests a number of factors which, insofar as they relate to the functional rather than the organic psychoses, appear to be insufficiently specified. They are:

1. Differences in occupational hazards (e.g., hazards producing brain injury or toxic psychoses).
2. Differential desirability of occupational roles, due to (a) operations performed or (b) place and conditions of work.
3. Differential prestige among one's fellows in the larger society.
4. Differing stresses due to financial problems and attendant dwelling locations.
5. Differences in mores and folkways regarding alcohol and sex.

Such "explanations" apparently do little more than point to concomitants of socioeconomic status. Except for certain obvious exposure differences (e.g., with respect to syphilitic, alcoholic, or toxic psychoses), they do not clearly indicate how the social class characteristics (e.g., low prestige

or undesirability of role) can produce psychoses.

Frumkin (14) has somewhat more elaborately indicated a possible functional relation between socioeconomic status and proclivity for particular types of mental illness. He suggests that such mental disorders as alcoholic and syphilitic psychoses, which occur most frequently among the "radical" lower status groups, are oriented primarily against society and its mores, whereas disorders such as manic-depressive psychosis, involutional melancholia, schizophrenia, and neuroses, which are relatively more frequent among the "conservative" upper classes, are oriented mainly against the self. In other words, lower class disorders are regarded as largely sociogenic, and thus generally oriented against society; while higher class disorders are primarily psychogenic, and thus generally oriented against the self. In addition to requiring a clearer distinction between sociogenic and psychogenic factors than can at present be demonstrated, this explanation seems to run afoul of certain available data on social class differences in diagnosis. Though the *ratio* of alcoholic psychoses to schizophrenia may be highest in the lowest social class, the absolute incidence of schizophrenia nevertheless appears higher in this group than in middle and upper classes. This is contrary to what one would expect from Frumkin's interpretation, if schizophrenia is in fact oriented primarily against the self. Social class differences in incidences of manic-depressive psychoses, involutional melancholia, and neuroses are not yet clearly enough established to be regarded as supporting the theory.

Sex Differences

Differences in mental illness attributable to sex differences are of

two kinds: differences in incidences of particular disorders, and differences in magnitudes of correlation of incidence with a third variable (an "interaction" of sex with the other factor). Data on first admissions to mental hospitals in selected areas of the United States indicate that rates are higher among males than among females for all psychoses except manic-depressive psychosis and involutional melancholia (50). This is especially true for alcoholic and luetic psychoses (13). Neuroses and psychosomatic disorders, on the other hand, are reported more frequently among women. This is said to hold specifically for asthma, migraine, urticaria, and hysteria, but not for peptic ulcers, where the sex difference is reversed (39).

With respect to the interaction effect of sex, Clark (3) reports a smaller correlation between occupational prestige and incidence of psychoses among women than among men. Schroeder and Beegle (53) found no urban-rural difference in Michigan suicide rates among females, whereas such a difference existed for males. The differing ratio of schizophrenia to manic-depressive psychoses among the various socioeconomic classes reported by Tietze et al. for the United States and Germany (38) held primarily for males, and to a much less extent for females. Finally, the correlations between business cycle and suicide-homicide rates reported by Henry and Short were stronger for males than for females (23).

Since sex differences are part physiological and part cultural it is not clear to what extent these effects should be assigned to organic factors and to what extent to characteristics of the male and female roles in our society. The specific relevant factors within either category have not been precisely elaborated. The "interaction" effect of sex differences reported

above appears to be consistently that of depressing the correlations with other known variables for females below the correlations found among males. This might be because women tend to be less closely tied to the particular roles which differentiate the men (e.g., occupations or urban vs. rural life); on the other hand, it might be due to a relatively more pronounced effect of organic, or other non-sociological, factors among women, and these various unassessed factors operate to depress the expected correlations with sociological factors.

Racial and Ethnic Differences

It is generally reported, on the basis of hospital admissions statistics, that psychoses, especially schizophrenia and manic-depressive psychoses, show higher incidence among Negroes than among whites (41, 50). However, there are some reversals of this difference (50), and it is not clear to what extent the general tendency is due to socioeconomic differences between the two races. No study has come to hand which compares incidence rates for the racial groups, while holding socioeconomic status constant.

A group of studies reported by Rose and Stub (50) indicate that Jews have lower incidences than non-Jews for all of the psychoses enumerated: schizophrenia, manic-depressive psychosis, alcoholic psychosis, general paresis, senile psychosis, psychosis with cerebral arteriosclerosis, and paranoia. A parallel difference does not hold for psychoneuroses under treatment, which may be somewhat higher among Jews than non-Jews.

There is an apparent interactional effect of race on the relationship of mental illness to other factors, similar to that discussed above for sex. Clark (3) reports that the (negative) cor-

relation between occupational prestige and incidence of psychosis is less pronounced for Negroes than for whites. Similarly, Henry and Short (23) indicate that the relations of suicide and homicide rates to the business cycle are different for Negroes than for whites. In the case of suicide, the Negro tendency is in the same direction as that for the majority group, but simply weaker. For homicide, however, the directions of relationship are opposed: homicides of whites increase during business contraction, while those of Negroes decrease.

Explanations for the differential rates of mental illness among the different racial and ethnic groups tend to be more speculative than empirically demonstrated. In addition to the problem of controlling socioeconomic status when making inter-ethnic and interracial comparisons, there is the likelihood that differential willingness to care for psychotics within the family, rather than within institutions, might be responsible for some of the apparent differences in incidence. Once such potentially confounding influences have been eliminated it might be worthwhile to explore the possibility that any remaining group differences be due to variations in subgroup norms or to differential security of the minority members within their own groups and within the larger society.

INTER-PERSONAL CORRELATES OF MENTAL HEALTH AND MENTAL ILLNESS

In this section we shall consider certain factors in mental health that have been explicitly defined in terms of group interaction processes. The causative nature of such factors has been implied in many of the explanations mentioned above for the various demographic differences reported.

But in the previous section, explanations in terms of group norms or other social processes have generally been offered as attempts to explain at a more basic level, and after the fact, relationships which had already been observed at a higher, or more superficial, level of analysis. The formulations to be considered here are more of a predictive sort, and imply that the presumably causative characteristics of all social interaction systems will have similar implications for mental illness or mental health, regardless of the specific demographic factors in which they happen to be manifest.

Though the determining characteristics of social processes to be discussed here are better formulated theoretically, there are frequently large gaps in the empirical data required to support them. Consequently, instead of being faced with large numbers of inadequately explained empirical correlations, as was frequently the case in the demographic sections, the reader may often find here intriguing theoretical formulations for which there is virtually no evidence.

In reviewing a number of studies and theories concerning the relation of sociological factors to mental health, Schneider (51) observed that nearly all their hypotheses could be subsumed under a small number of categories. Mental disturbance was seen variously as stemming from (a) the individual's inability to meet role demands, (b) membership in some underprivileged group, (c) a forced abrupt transition from one social situation to another, (d) disorganization of a social system, (e) inability to attain social acceptance, (f) subjective or objective mobility in the class structure, (g) incompatible values, and (h) social isolation.

Here we shall discuss these and

other factors presumed to affect mental health under three headings: First, there are factors reflecting the degree to which the individual maintains group membership ties, or the relative strength of those ties. Second, there are factors which describe the nature of the intragroup relations in which he is involved. Finally, there are factors relating to conflict or compatibility among the various groups of which he is, simultaneously or successively, a member. Each of these categories of supposedly determining characteristics will be discussed in turn.

Extent of Group Membership Ties

It has been maintained by some (11, 37) that social isolation, in and of itself, is a causative factor in the development of mental disorder, especially schizophrenia. From a contemporary analysis, the hypothesis may be stated as follows: low participation in social interaction, either through isolation from one's membership and reference groups or through rapid mobility, results in socially deviant behavior and accompanying disorganization of psychological processes (5). Or, formulated developmentally, if a child's interaction with his peer group is restricted by his parents, his understanding of other people's reactions never develops; consequently his own responses to social situations are inappropriate (11).

A number of cross-sectional correlations have been reported which are consistent with this formulation, but they are not specific to schizophrenia. Faris and Dunham (6, 12), in their original ecological study of Chicago, found admission rates for schizophrenia to be disproportionately high among persons from areas which were not primarily populated by members of their own ethnic or racial groups.

That is, admissions rates tended to be higher for minority members of a community than for members of the dominant group. Lemert (37) found in rural Michigan a negative correlation between the incidence of hospitalized psychosis (undifferentiated) among foreign-born people and the number of foreign-born of that nationality living in the county. Gruenberg (20) reports that Norwegian immigrants in Minnesota had higher admissions rates than native-born in the same area, and higher rates than Norwegians who stayed in Norway. Also that the native white rate of admissions is higher in a predominately Negro area than the rate for Negroes in the same area, while the Negro rate in a dominant white area is higher than the white rate.

In his study of the distribution of senile psychoses (admitted to hospitals) in Syracuse, Gruenberg (19) found that the number of people living alone in a particular area was a better predictor of incidence than was the socioeconomic status of the area. Similar correlations with both of these factors were also found for all other admitted psychoses (undifferentiated).

With respect to suicide rates, a social isolation interpretation is also applicable. Suicide occurs more frequently among unmarried persons, especially divorced, than among married persons (10, p. 147). Immigrants are said to have higher suicide rates than natives, and higher rates than persons who did not emigrate from their nation (10, pp. 148, 161).

Finally, certain neurotic anxiety symptoms were found by Halmos (21) to be associated significantly, though moderately, with reported difficulties in social interaction and with actual indices of the extent of the subject's social participation.

It is difficult to evaluate the etio-

logical significance of social isolation on the basis of these correlational findings, because of the large number of uncontrolled factors. Since the immigrant population is largely self-selected, it may differ systematically from native-born and nonmigrants in their own countries with respect to factors predisposing to mental illness. And the choice of place of residence—whether with people of one's own ethnic group or not—is to a certain degree a matter of self-selection, so the direction of the relationship between isolation and proclivity to mental disorder is hard to determine. Moreover, since social isolation of various sorts has been found associated with a variety of mental disturbances—schizophrenia, senile psychosis, suicide, and anxiety symptoms—the specificity of such an etiology to schizophrenia is hardly established.

Two available studies attempted to distinguish schizophrenia from manic-depressive psychosis in regard to social interaction correlates. They lead to opposite conclusions concerning the specificity of social isolation as a causative factor in schizophrenia. Jaco (29) determined the incidence rates of the two disorders in 15 census tracts in Austin on the basis of commitments to a local public mental hospital. He then selected the two tracts which were highest for each disorder, and compared them, respectively, with the two tracts lowest for each disorder on a number of social characteristics such as anonymity, spatial mobility, social participation. These characteristics for the tracts were determined either from census data or from systematic sample interview surveys within them. With respect to some of the indices the high and low manic-depressive tracts were found to differ significantly in the same directions as the

high and low schizophrenia tracts. When such parallel differences occurred, the significance of the difference between differences was tested (high vs. low schizophrenia areas minus high vs. low manic-depressive areas). Those indices of social isolation which differentiated residents of the high and low schizophrenia areas to a greater degree than the high and low manic-depressive areas were: (a) likelihood of knowing the names of one's neighbors, (b) number of friends, (c) number of acquaintances, (d) membership in lodges and fraternal organizations, (e) unemployment rate, (f) number of jobs held previously, (g) number of visits to central business district, (h) number of visits with friends, (i) number of visits to other areas of the city, (j) number of trips out of town, (k) amount of intercity migration (high schizophrenia tracts showed less), (l) number of friends in remote areas (high schizophrenia areas less). Differences specific to schizophrenia rates were *not* found for the following characteristics of the tracts: (a) church membership, (b) voting in previous election, (c) frequency of discharge from, or quitting of, jobs.

These general differences among the census tracts are interpreted by Jaco as evidence in favor of the specific applicability of the social isolation hypothesis to schizophrenia. Yet there is a statistical flaw in the study which may have contributed to spurious differences, and hence renders the interpretation questionable. When comparing high- and low-incidence tracts, significance tests were based on the size of the sample of respondents, with no correction for homogeneity of people with tracts. The appropriate test would have been to consider the sample as a multistage sample of people within tracts, and base the significance tests on the

number of clusters, i.e., two high-rate tracts and two low-rate tracts. Since the wrong statistical tests were employed, it is reasonable to infer that significance levels were overstated—this assumes positive intraclass correlations for the traits measured. (For a discussion of multistage sampling and the effects of clustering, see Kish [31].)

This criticism may be rephrased in nonstatistical language as follows: The four tracts compared on the social isolation characteristics were not selected in random fashion from larger populations of high and low schizophrenia tracts. Hence, other differences among the tracts, besides those relevant to the hypothesis, cannot be assumed to be randomly distributed. One does not know, then, to what degree the differences in incidence of schizophrenia are associated with the designated characteristics and to what degree with contaminating factors. Thus, although people in the two selected high-incidence tracts differed significantly in certain respects from people in the two low-incidence tracts, it is not possible to generalize these differences beyond the four particular tracts included in the sample. Whether or not these obtained differences are critical to the incidence of schizophrenia in other communities—even within Austin—is not ascertainable from this study.

Kohn and Clausen (33) obtained interview data from 45 schizophrenic and 13 manic-depressive patients from Hagerstown, Maryland, who were hospitalized between 1940 and 1952. This was a nearly complete population, rather than a sample; exclusion of people from the interviewed group was due to inaccessibility (physical or psychological), rather than to random selection. These 58 patients and ex-patients were matched, subject for subject,

with a normal sample, on the basis of age, sex, and occupation. Both the schizophrenic and manic-depressive groups were more likely than matched normals to have been relatively isolated at age 13-14 (that is, larger proportions of them played primarily alone at that age). But the schizophrenic and manic-depressive patients did not differ from each other in this respect. Moreover, the interview data yielded no significant differences between patients' and normals' reports of their parents and of interpersonal relations within their families of orientation. Specifically, there was no evidence that social isolation resulted from parental restrictions. The authors are led by these findings to propose an alternative explanation to the standard social isolation hypothesis: As a result of inadequacies in social relationships, certain individuals come to feel that they do not belong to their peer groups. This may lead to withdrawal, but not necessarily; it may also lead to compulsive interaction of the kind engaged in by some manic patients.

In assessing the adequacy of this study's findings for its conclusions, two features of the design may be considered (aside from the restriction of the sample to the particular hospital and time period). First, it should be noted that the samples were quite small (13 manic-depressives, 45 schizophrenics), so that small differences between these groups were not likely to appear statistically significant. Second, there is a difficulty inherent in *ex post facto* matching. Since it is not possible to match on the basis of all possibly relevant variables, one cannot be sure that differences between patients and normals did not result from some contaminating factor which was not controlled. In this particular instance, a crucial contam-

inating factor might have been the distortion of recall concerning the patients' childhood play relationships. Thus the results of this study serve rather to suggest hypotheses for more systematic exploration than to test in unambiguous fashion previously established hypotheses.

The strength of group membership ties can be assessed on bases other than simple social isolation. Some groups, by virtue of strong normative forces, exert more pressure toward conformity on their members than do other groups. Durkheim observed this in his interpretation of differing suicide rates in different membership groups (8). Henry and Short (23) have generalized and extended this interpretation in attempting to account for differing suicide and homicide rates among various demographic groups, and for differing magnitudes of correlation with the business cycle of suicide and homicide rates in these groups. Their formulation is intriguing and exceedingly general, though it gains only suggestive, rather than conclusive, support from the demographic data brought to bear on it.

Assumptions of Henry and Short's interpretation are that: (a) the basic and primary target of aggression is another person rather than the self; (b) the degree to which aggression is directed against others varies with the degree to which other-oriented aggression is defined as legitimate by the aggressor. Self-oriented aggression thus becomes a residual category—aggression for which outward expression is denied legitimacy. Proceeding from these assumptions, they theorize that, when behavior is required to conform rigidly to the demands and expectations of others (i.e., when external restraints are strong), the expression of aggression against others tends to be legitimized. When external restraints are weak,

other-oriented aggression consequent to frustration fails to be legitimized, and aggression is directed against the self.

This reasoning suggests the hypotheses that the tendency toward suicide should vary inversely with the strength of external restraint over behavior, while the tendency toward homicide should vary directly with the strength of such restraint. Two principal sources of external restraint are posited: low social status and a strong relational system. Thus a low status person's response to frustration is more likely to be homicide than suicide; this differential proclivity is reversed for high status persons (e.g., white, male, young, wealthy). Also for people in groups with strong relational systems (e.g., nuclear family, Catholic church) the tendency toward homicide as a response to frustration should be greater than that toward suicide; while the reverse should be true for members of weak relational systems.

Such an interpretation helps to rationalize a number of diverse empirical relations between suicide and homicide rates on the one hand, and certain environmental and demographic factors on the other. For instance, suicide rates tend to increase during periods of business contraction (suicide representing a high status response to frustration). This correlation is stronger for high status groups—males, whites, younger persons, and residents of high rent areas—than for low status persons. Homicide rates of whites tend to increase during business contraction (external restraints are increased for the dominant group), while those for Negroes tend to decrease (external restraints are decreased for the outgroup).

This model is intriguing for its generality and simplicity, yet one should bear in mind that it was de-

veloped largely as an attempt to explain already existing data. Hypotheses using new indices of the major variables (status and strength of relational system) will be required to establish its predictive validity. Moreover, while the theory refers to interaction processes (degree of external restraint over behavior), the data brought to bear on it come from higher units of analysis—census tracts and other demographic categories. So one is left with questions similar to those applicable to the ecological studies reviewed earlier: Have the presumed causal relations been directly tested? (Apparently not.) Can the empirical results be attributed to genotype factors other than those specified by the theory? (Probably so.)

The strength of group membership ties may be regarded as a function of the rigidity of group norms, but the degree to which these impinge on the individual will also be a function of his degree of identification with the group. Accordingly, one might expect various indices of mental health to be lower for transitory or highly mobile members of a community than for the more stable members. Such has been found to be the case in at least two studies of treated psychoses. Tietze et al. (60) report higher rates of mental disturbances in the Baltimore Eastern Health District among people who move most frequently (holds for intracity migrants only). Hollingshead et al. (24) found more upward social mobility among their Class III (middle class) schizophrenic patients than in the cross-sectional sample of New Haven. (Class V patients—lower class—did not differ from the total community in this respect.)

In the realm of nonpsychotic mental disorders, Ellis (9) reports a similar relation between upward

mobility and evidence of emotional maladjustment. She analyzed data from 27 single working women in Montgomery, Alabama, whose occupational prestige ratings were higher than their fathers' (upwardly mobile), in comparison with 33 single working women whose occupational prestige ratings were the same as, or lower than, their fathers' (nonmobile). Larger proportions of the upwardly mobile reported experiences of community and parental rejection in childhood and of parental preference for a sibling. They tended more often to report their own attachment to parents as "less than average." At the time of the study, these upwardly mobile women had fewer intimate friends and a shorter average duration of friendships than the stable group. More of them kept pets and more manifested psychosomatic symptoms. There was no difference between the groups in self-report of happiness.

Ellis' conclusion is that "The evidence is consistent with the theory that upward social mobility is likely to be an outgrowth of basically neurotic drives resulting from unsatisfactory early primary group relations, and that mobility leads to a continuation of superficial, impersonal primary group relations and other overt manifestations of emotional maladjustment" (9, p. 563).

This interpretation explicitly posits a mutually reinforcing relation between mental disturbance and mobility, and does not assign causation exclusively in one direction. A similar circular effect may be presumed to hold for the psychoses. As Dunham (7) has pointed out, whether schizophrenically disposed persons move frequently or whether frequently moving persons are more prone to develop schizophrenia is an open question.

One might expect that the disruptive effects of status mobility could be counteracted by enduring primary group ties for the mobile individual. Thus there should be less difference in the mental health of mobile and nonmobile married persons. Mangus' study of personality changes in adolescents over a 10-year period (42) showed relative deterioration among children from broken homes, but relative improvement among children of upwardly mobile families. The latter findings might be interpreted to suggest either that the upwardly mobile parents were *more* healthy than average or that they were less likely than others to transmit their maladjustments to their offspring. However, since Ellis' and Mangus' studies employed rather different criteria for mental health, a comparison of their findings is inconclusive with regard to the interactive influences of primary group ties and status mobility.

The effect of group membership ties in restoring mental health, as well as maintaining it, is suggested by Straus (57), who describes the experience of the Blue Hills Hospital (Connecticut) in its outpatient treatment of alcoholics. Patients who were both married and employed were twice as likely to continue therapy following hospitalization as were those who were neither married nor employed.

The implication of many of the foregoing theories and data regarding the effect of group membership ties on mental health appears to be that membership *per se* contributes to sound mental health. The overgenerality of such a conclusion is apparent if one considers that the norms of certain groups may be such as to impair mental health, either from the standpoint of adjustment within the wider society of which the group is part, or from the standpoint of cer-

tain "absolute" standards of mental health which the researcher may espouse. It has been noted by sociologists and social psychologists (40, 45, 58, 62) that delinquency and criminal behavior are supported by the norms of deviant groups with which the individual identifies. Knowledge and beliefs in this area are based more on anecdotes and case history studies than on empirical testing of systematic hypotheses. One is thus left, at present, with the rather trite generalization that the degree to which group membership contributes to mental health depends in part on the relation of the norms of the particular group to the criteria for mental health which one adopts.

Another limiting consideration is suggested by Jahoda (30), to the effect that the degree to which membership in any one group promotes mental health is a function of the broader community structure within which that group operates. She hypothesizes that if a community is organized so as to favor mutually exclusive voluntary group memberships, the dependency of the individual on the one group to which he belongs will be excessive. By contrast, when an individual can belong to several groups which put less exclusive claim on his loyalty, so that his identity will not be threatened when he leaves the group, membership in it will enhance his mental health. The evaluative implication of this hypothesis is that, while group membership may contribute to the security and ego identity of the individual, these aspects of his self should not be totally dependent on a single group. Such an implication appears completely appropriate to the kinds of criteria for mental health which Jahoda has proposed, namely "active adjustment," "need-free perception," and "integration of per-

sonality." It may well be relevant to mental health as defined by other criteria as well, but data bearing directly on this hypothesis are not available at present.

Such considerations as these force recognition of the possibilities that whether, and to what degree, group membership enhances or impedes mental health depends on: (a) the nature of the group and its relation to the wider social structure, (b) the manner in which group membership is maintained by the individual, and (c) the particular criteria for mental health chosen. Further systematic elaboration and testing of these notions would appear fruitful.

The Nature of Intragroup Relations

Certain propositions concerning the effects of group structure on mental health have been advanced, both at a general level and with reference to a specific group, the family of orientation. Data on the general propositions are hard to come by, and those relating to the specific propositions are inconclusive.

At the general level, it has been maintained, on the one hand, that the degree of adjustment to roles is a function of the degree of clarity with which they are defined (30). Somewhat the same idea is involved in the proposition stemming from Durkheim (8): Where roles are infinitely extensible, disorientation (anomie) occurs, because no limits are set to human desires. On the other hand, where roles are nonextensible, disorientation may occur through a failure of the role to absorb more than a small portion of the personality; the personality is thereby blocked and thwarted (51). This is stated somewhat more specifically by Jahoda in the hypothesis: If a single type of behavior is rewarded, the pressures for conformity—that is, passive accept-

ance, rather than active adjustment—will be great, resulting in mental illness by her definition (30). Thus, the likelihood of breaking down under a specific role may be expected to vary inversely with the number of roles one plays in a given system (28, p. 46).

Combining these two sets of propositions, one might propose that poor role definition promotes maladjustment (at least within the particular social system in which the role occurs; not necessarily within a different, or more inclusive, social system), while overprecise role definition, if not accompanied by available alternative roles, may lead to mental illness, not from the point of view of adjustment to the particular social system, but from the standpoint of some other, system-free, criterion of mental health. Thus optimum mental health, by both kinds of criteria—social and psychological—might be expected to occur to the extent that roles are clearly defined, but in a way which does not demand exclusive absorption of the person within them.

At a level more amenable to empirical testing, hypotheses regarding the effect of intrafamily relations on children's mental health have been proposed. Wahl (61) suggests an intrafamilial etiology of schizophrenia as follows: The child's attitude toward himself is a function of his parents' relations with him. In large families, children are more frequently ignored than in smaller families, thereby engendering feelings of worthlessness, presumably a predisposing factor in schizophrenia. This is suggested as an explanation for data presented by Wahl to the effect that a sample of schizophrenics in Elgin State hospital came from families averaging 4.1 children, in comparison with the U. S. average of 2.2 children per family. A major short-

coming of these data in reference to the hypothesis is that they fail to take into account differences in social class distribution between the patients and the general population. Schizophrenics appear to come preponderantly from the lower classes, for reasons not yet clearly understood. Perhaps the relatively large family size in these groups is a crucial factor but, if so, then the relationship between family size and prevalence of schizophrenia should be tested *within* each social class, rather than within the total population at once. A partial correlation between family size and prevalence of schizophrenia, with social class held constant, would provide more convincing evidence in favor of the hypothesis.

Failure to control for social class differences might lead to unwarranted conclusions with regard to another characteristic of family structure, the locus of parental authority. In a study by Kohn and Clausen (34), comparing 45 schizophrenic patients and ex-patients from Hagerstown, Maryland, with a "normal" group matched on age, sex, and occupation, more of the schizophrenics reported that their mothers played a dominant role in the family decision-making process. But when socioeconomic status was controlled by testing this relationship within each of three social class groups, the difference between schizophrenics and normals disappeared, except for the higher SES group. It is probably not justified, on the basis of this analysis, to conclude that maternal dominance plays any role whatsoever in the etiology of schizophrenia, even in the upper class group, for singling out one significant relationship among the three tested tends to capitalize on chance variation to a greater degree than is indicated by the reported level of significance.

A study in near-publication stage, by Miller and Swanson (43), is expected to shed some light on the social psychological mechanisms underlying social class differences in mental disorders. These investigators proceeded from the assumption that differences between middle and lower class child rearing practices would produce differential preference for defense mechanisms relevant to manifestations of mental illness. Thus a conceptual link is made from characteristics of social structure, through interpersonal relations, to psychological mechanisms.

In the Yorkville community mental health study (48) the following aspects of relations within the respondent's family of orientation were assessed in the interview: (a) the degree of interparental accord or discord, (b) parental anxiety level, (c) degree of affectional acceptance or rejection from parents, (d) parents' predictability in punishing misdeeds, (e) parents' pressures on the respondent for goal achievement, and (f) extent of sibling rivalry. Hypotheses concerning the influence of such factors on mental health are only implicit in reports of this study to date. Results of their testing are scheduled for publication in the near future.

A recently completed investigation (15, 16, 17) of the relation between parental attitudes and children's adjustment in St. Louis has yielded negative findings in this regard. Employing a battery of questions similar to those previously validated by Shoben (56) in his study of juvenile delinquents, Gildea, Glidewell, et al. attempted to relate mothers' attitudes toward child rearing to the level of adjustment shown in their children's classroom behaviors. No correlation appeared between responses to Shoben's items and emotional disturbance of the children.

There was, however, a substantial relation between social class and these child-rearing attitudes. Moreover, certain other parental attitudes which were previously believed by these researchers to relate directly to the children's mental health—such as acceptance of responsibility for behavior of one's children—were subsequently found to be highly related to socioeconomic status. When the latter variable was held constant, the predicted correlation between mother's attitude and child's behavior vanished (15).

Caution must be exercised in interpreting these findings to indicate that child-rearing practices related to these attitudes have no effect on children's adjustments independent of social class. It is evident that the attitudes expressed in response to the questionnaire items may in many cases bear scant resemblance to the actual nature of the parent-child relationships. The fact that upper and middle class respondents give more "acceptable" answers to the questions may indicate, more than anything else, that they know what sorts of responses a middle class interviewer will approve.

A similar kind of limitation of their own interview study has been considered by Sears et al. (55). They minimize its importance, on the basis of internal evidence from their data, and suggest a number of relations between child-rearing practices and emotional disturbance in infants. Among them are the following: severity of weaning and toilet training are associated, respectively, with the extent of the child's upset over weaning and toilet training. The latter response is *not* associated with more general training practices, such as severity of restrictions or achievement demands. Retarded bladder control in children is associated with

a combination of severe toilet training, high maternal sex anxiety, and "cold personality" in the mother. Excessive feeding problems were found with a combination of severe toilet training, physical punishment, and maternal "coldness." All data on both training practices and emotional disturbance were obtained from the same interview with mothers several years after many of the relevant events had occurred. Thus, while they provide important information concerning interrelations among contemporary attitudes, perceptions, and remembrances, they offer less clear evidence concerning the effects of early family relationships on later personality patterns. Definitive studies in this area would probably have to combine direct observation of child-rearing practices with a subsequent independent assessment of the child's level of mental health.

Compatibility among Norms and Roles of Different Groups

It is generally maintained by social psychologists that the individual's personality is largely created through his performance of prescribed or idiosyncratic roles in the various groups in which he participates.

Insofar as socially important roles form a portion of the personality system and insofar as action within these socially important roles is motivated by the interiorization of certain cognitions, beliefs, values, and attitudes, personality system and social system may be said to "mesh" or be oriented (51, p. 35).

Disorientation between the personality and society is thus defined by Schneider as a failure of the individual's motives, attitudes, beliefs, values, etc., to mesh with the needs of certain socially important roles (51, p. 36).

Such a failure of congruence may be attributed to the fact that the system of norms and roles within which the personality was formed is frequently quite different from that to which the person is required to adapt in adult life. Presumably slight or moderate changes in the individual's social system will not lead to incongruence, for the personality is capable of becoming adapted, through learning mechanisms, to a reward structure somewhat different from that to which it has been accustomed. However, extreme discontinuities requiring adaptation to a diametrically opposed set of norms may produce such violent disruption of expectations that new learning fails to occur. Some exceedingly basic and general dimensions of role definition have been described by Parsons (46); Schneider applies these to a theoretical formulation of some possible sources of disorientation between the personality and the social system.

Disorientation may be expected to occur when: (a) the individual's need for affection and response are not met within the cold impersonality of many of the institutional roles of the large-scale society; (b) a personality incorporating dominant collectivity-oriented motivation is unable to meet the demands of ruthlessly individualistic roles; (c) a personality which has been formed within the framework of ascribed roles is unable to adjust to the demands of achievement; (d) a personality which has been formed largely within particularistically oriented roles, which accentuate affectional needs and emotional dependence, meets the universalistic roles of an industrial urban society. More specifically, Schneider hypothesizes that:

1. Occupations in which competi-

tion is exceptionally severe and judgments of attainment highly impersonal (e.g., bureaucracy) will be marked by high incidence of mental disorder.

2. Mental disorder will result when an affectively oriented person encounters severely oriented roles.

3. Mental disorder will occur when the support of affectively charged relations is suddenly removed, for example, by a broken home or a disrupted love affair.

4. Mental disorder will occur when members of a minority group encounter severe amounts of hatred or discrimination, especially when their personalities predispose them to feel the same type of prejudice against their own group that they themselves are experiencing.

It is generally assumed that, in Western society, the nuclear family and other primary groups contributing to socialization of the child partake of the *Gemeinschaft* characteristics—*affectivity*, *collectivity-orientation*, *ascription*, *particularism*, and *diffuseness*—while the secondary groups of later life display the opposite, *Gesellschaft* characteristics—*affective neutrality*, *self-orientation*, *achievement*, *universalism*, and *specificity*. In a folk society, where *Gemeinschaft* characteristics are more prevalent in all roles, discontinuity would presumably not be a dominant source of mental disorder (51, p. 48).

The implication of Schneider's formulation would appear to be that nearly all members (or at least male members) of this society, reared as they are within primary groups, would be expected to experience some degree of disorientation to certain of the social roles they are expected to perform as adults. "Disorientation" seems to be regarded by Schneider as

equivalent to mental disorder, and in this sense the formulation appears extreme. While disorientation may give rise to mental disorder (in the more common senses of the term), the latter should not be regarded as either synonymous with, or a necessary concomitant of, the former. Human beings have mechanisms for handling disorientation, not the least prevalent of which is the kind of "active adaptation" to which Jahoda refers (30).

One is tempted to suggest that there are more exceptions than corroborations of the propositions put forth by Schneider. In fact, one might expect that incumbents of the complex, competitive, impersonal roles typified by bureaucracy would have developed a correspondingly complex set of defensive or adaptive mechanisms which serve to maintain personality integration in the "normal stressful situation" (22) encountered in such roles. Perhaps the roles themselves are a source of adaptive mechanisms as well as stresses. Thus a more complex set of propositions is called for: The likelihood of disorientation varies as a function of discontinuities in role expectation along the dimensions suggested by Schneider, but mental disorder as a response to such disorientation is likely only to the extent that adaptive mechanisms are not either already available to the person or acquired by him in the course of role performance. From this point of view, the crucial factor in mental disorder is not so much the fact of disorientation between the personality and social system, as it is the manner in which such disorientation is handled by the personality.

Jahoda (30) has advanced two hypotheses concerning the learning of adaptive mechanisms through the socialization process:

1. The correct perception of reality will increase through frequent changes of environment; this will, at the same time, be conducive to passive acceptance, rather than active adjustment, unless the original "home" environment has enhanced the integration of personality.

2. The greater the chance to practice active adjustment and to reject passive acceptance at an early stage, the less is the likelihood that environmental changes will produce passive acceptance.

SUMMARY

This review of empirical and hypothetical correlates of mental illness has classified the relevant theories and findings into two broad categories. The demographic-environmental category includes a large number of empirical studies which relate the prevalence or incidence of mental disorder to diverse ecological and cultural factors. In most instances the researchers used hospital records as sources of data, so the operational criterion of mental illness was generally that of exposure to psychiatric treatment. The limitations of such a criterion have been discussed elsewhere (54). Moreover, such interpretations as have been applied to these findings are generally of an *ex post facto* sort, not subjected to direct tests of the presumed critical determining factors.

In the second category, interpersonal correlates, have been included a number of theories and some empirical findings concerning the significance of social interaction processes in the development of mental disorder. A variety of operational definitions of mental illness have been used in these studies, but the one which appears conceptually appropriate to most of the formulations

is that of maladjustment, or deviance from social norms. Thus the researcher who would coordinate theory and empirical data within and between these two levels of analysis is faced with the problem of determining the relation between social maladjustment and exposure to psychiatric treatment—two distinct definitions of mental illness. Though some correspondence between these two kinds of criteria has been indicated (54), it is not clear that inclusion by one necessarily requires inclusion by the other.

If we assume what has yet to be demonstrated, that social maladjustment will eventuate in mental disturbance diagnosable by other criteria, it is possible to speculate regarding a possible correspondence between some of the demographic-environmental correlates and some of the interpersonal correlates of mental illness. Specifically, we may interpret certain empirical relations in the former category in terms of concepts utilized in the latter. It is possible, for example, to regard major cultural events, such as war, depression, and technological advancement as having potential effects on the society's mental health through the widespread role dislocations and disruption of primary group ties which accompany them. Similarly, it is possible to seek explanations for the geographic, socioeconomic, race, and sex differences in rates of mental illness in certain interpersonal characteristics of the ecological and demographic situations. Possibly relevant dimensions of the interactional processes have been suggested: social isolation, the degree of involvement of individuals within a particular social system, and differences in socialization procedures employed by various groups.

Such interpretations as these are

speculative and vaguely formulated at present. It is by no means clear that the interpersonal level of analysis is the most appropriate level at which to seek explanations of mental disorder. Nevertheless, it appears likely that a reformulation of some of the macro-societal differences in micro-social terms will suggest genotypes which are not readily discerni-

ble at the grosser levels of empirical correlation. Even if these do not provide causal explanations for many of the psychological manifestations which accompany mental disorder, they may at least expand the field of inquiry in directions which could eventually yield more basic understanding of the nature of mental health and mental illness.

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CONSIDERATIONS IN THE ANALYSIS OF GROUP TASKS¹

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One of the major developments of recent psychological research is the extent to which important problems in the area of small-group behavior have been translated into laboratory operations. Many aspects of small-group behavior which, a few years ago, would have been investigated through survey techniques or observation *in situ* are now subjected to the relatively more precise and definitive treatment which laboratory investigations provide. However, as Kelley and Thibaut have noted (23), this broadened conception of the potential range of experimental manipulations has been accompanied by a shift in emphasis from group products to group processes; problem-solving groups have been displaced largely by discussion groups having less explicit goals, with the investigator attending primarily or exclusively to reactions of the members to each other.

One consequence of this orientation has been a lack of systematic attention to the problem of isolating and defining the characteristics of task which govern group behavior. Since the task has usually been construed only as a device for insuring a certain level of social interaction among group members, refined qualitative distinctions among tasks have

presumably appeared somewhat irrelevant. It is not surprising, therefore, that the classification and organization of task properties has not kept pace with the general progress of small-group laboratory research.

Recently, however, research in military settings has re-emphasized the importance of "action" groups. Such groups, generally designated "crews" or "teams," are formed to accomplish some explicit mission, typically requiring a high degree of interaction between the environment and the team members. Even a casual acquaintance with these groups indicates the diversity and complexity of their task environment and the high degree of dependence of group action on the nature of this environment. Thus the analysis and investigation of task characteristics becomes a basic requirement for research directed toward producing generalizable principles concerning the structure and functioning of military groups.²

The major purpose of the present report is to propose a paradigm that

² Several other major efforts in this direction have come to the attention of the authors while this manuscript was in preparation. One of these is being conducted by Psychological Research Associates under the direction of Joseph E. McGrath and the other study was performed by the American Institute of Research under the direction of Murray Glanzer and Robert Glazer. Although both of these projects had rather different functions and points of departure from the present investigation, there seems to be an encouraging degree of agreement as to the essential properties of task and as to prominent gaps in current small-group research.

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may be useful in preliminary attempts to isolate and define important group-task characteristics. Based on this paradigm, several classes of relatively molecular differentiating properties of tasks will be discussed. Following this we introduce the concept of "critical demands"—task properties at a higher level of abstraction which we believe may serve as useful intervening variables between molecular task properties and task performance measures.

THE DESCRIPTIVE PARADIGM

Task performance usually involves a chain of events in which physical processes and response actions by group members interact in complex fashion. Although this entire chain of events is responsive to the joint experimental operations of physical task design, subject selection, and task instructions, any discernible relationships with those operations undoubtedly become highly attenuated. For this reason it is proposed, in light of the present state of knowledge, to give equal status to "objective" task properties and to what will be referred to as "modal" task properties.

An *objective* property is one for which an experimenter can specify a single, definite, value by suitable measurement or control. The number of display instruments would be an example. A *modal* property reflects "typical" behavior of individuals or task variables but is subject to variation due to group characteristics and their interaction effects with other task properties. An example would be the frequency of occurrence of certain response classes. In general, modal properties may be indirectly manipulated by the experimenter but they are not, strictly speaking, "independent variables."

Figure 1 presents, in schematic

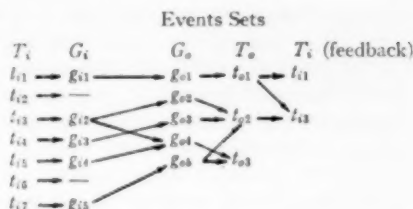


FIG. 1. A PROPOSED PARADIGM FOR THE DESCRIPTION OF GROUP-TASK SYSTEMS

form, the four-stage cycle of task events on which the subsequent elaboration of group-task properties will be based:

1. Some set of events (t_{i1} , t_{i2} , t_{i3} , ...) occurs in the general surroundings of the group. The entire class of such events is denoted by T_i (i.e., the class of *task input variables*), and might include such things as variations in input displays, stress-inducing stimuli, etc.

2. A correlative set of events (g_{i1} , g_{i2} , ...) occurs within the group. We designate the class of such events as G_i (i.e., *group input activities*). It consists of attending or observational responses on the part of group members, and of communication processes relating to input information.

3. Following the occurrence of the activities in G_i , there is a set of events (g_{o1} , g_{o2} , g_{o3} , ...). The class of *group output activities*, G_o , includes such things as decisions to make certain motor responses and the motor responses themselves.

4. There then occurs a further set of events (t_{o1} , t_{o2} , t_{o3} , ...) in the external environment surrounding the group. This final set is denoted by T_o (i.e., *task output variables*). Some subset of events in T_o generally forms the basis for evaluation of group performance.

Thus we assume two classes of events which occur in the immediate environment of the group and two classes of behavioral events or ac-

tivities. The distinction between "input" and "output" can probably not be defended as a clear-cut dichotomy, but it is useful in that it permits the description of many important task characteristics in terms of relations between input and output events.

For describing each of these four classes of task events, T_i , G_i , G_o , and T_o , three general types of property seem pertinent. First, there are *descriptive aspects* which concern the qualitative nature of the events as well as their numerousness and possible metric properties. The second type of property is concerned with the *distribution* of the component events in physical space or with respect to other events. The third type of property is perhaps more complex and revolves about the *functional behavior* of the events—that is, their occurrence over time or as a result of preceding events. The four-by-three breakdown implied by the four event classes and three types of event property is used as the generating framework for a molecular task description. The following description of event sets, however, will not attempt to be exhaustive but will place primary emphasis on those properties which may affect group operations directly rather than through a modification of individual performance. Fuller treatments of task variables presumed or known to influence individual performance are presented by other investigators (e.g., 1, 2, 15, 16, 17, 22).

MOLECULAR EVENT SET PROPERTIES

T_i Properties

Descriptive aspects. Task systems vary tremendously in the number of environmental events having potential stimulus or information value. In group experiments using the autokinetic phenomenon, for example,

there is a single stimulus input, whereas tasks attempting to simulate operational systems must provide for a great number of input variables. Occasionally, it may be possible to "dimensionalize" input variables to take account of overlap in actual information conveyed, but this would be very difficult for some tasks.

Display inputs are further characterized in terms of such qualitative properties as: the sensory modalities for which the T_i variables constitute adequate stimuli; stimulus ambiguity and thresholds of discriminability; and background and contextual factors. The importance of such characteristics from the point of view of the analysis of group-tasks is that they affect the reliability of certain observations or reports by group members, and thus, indirectly, may influence the structure or functioning of the group. As an example, for particularly ambiguous stimuli, observations by several group members may be required to achieve some necessary degree of reliability and the group would have to be organized to provide overlapping coverage.

Distribution. Input stimuli may be uniformly accessible to all group members or their availability may be restricted by physical or administrative means. In such tasks as story reconstruction (33), syllogistic reasoning (10), the intercept task (25), and some of the network problems (6, 28, 29), information is completely distributed. For the mechanical assembly task (8), a single diagram makes simultaneous access to a primary input very difficult. Shaw and others (18, 42) have used tasks in which differential access to information is deliberately manipulated.

Functional behavior. In the great majority of laboratory studies, task input variables are all introduced at the outset and are unchanged there-

after. However, several studies, such as those of Moore and Anderson (34), and a series by the present authors (24, 25, 26, 27, 37), have used changes over time. If the input is extended over time, the changes in task variables may be characterized in terms of rate of change (e.g., bits per minute), continuous versus discontinuous change, and periodicity (i.e., whether there are cyclic recurrences of stimulus values). There has been little systematic study of these parameters for group tasks although they have been widely investigated in individual tasks (31).

T_i events may be functionally related to preceding T_o events either mechanically or by means of experimenter operations (14). In a problem-solving task described in a recent report (39), signal light displays are varied by pushbuttons at the control of the subjects so that a perfect "closed loop" exists. More typically, however, T_o - T_i relationships are lagged or diffused so that little feedback occurs until a task trial is completed. Where such terminal feedback occurs, it is usually of a reinforcing or evaluative nature. A study by Berkowitz and Levy (9) demonstrates the consequences of evaluative feedback depending on whether it is directed at specific group members or at the group as a whole.

G_i Properties

Descriptive aspects. G_i activities in general center about the process of collecting and disseminating information. In terms of motor activity, the collection of information may involve nothing more than watching a given instrument (37), or reading prepared materials (18); in other tasks (there are no known examples in the experimental literature) extensive scanning or active reconnaissance might be required. At the conceptual level, ob-

servations may be of instantaneous conditions of T_i variables or they may require the detection of trends or cumulative values of the variables, analogous to differentiation or integration. As an example of integration, the intercept task (25) required subjects to count "enemy planes" entering an area in order to determine how many "interceptors" to send out.

G_i activities may be wholly concerned with T_i events or they may have little or no relationship to the immediate task environment. A nearly pure case at the latter extreme is the discussion task in which various group members introduce "inputs" stemming from their previous reading or experience. By a liberal interpretation of our definition, procedural suggestions or social-emotional material might also be placed under this heading.

Distribution. In the case of G_i (and G_o) events, "distribution" may be interpreted as the allocation of activities to persons or to specific physical loci. Specialization of G_i functions may be imposed administratively or may arise as a natural adaptation to the variety and spatial distribution of T_i variables. Group members may specialize in terms of the T_i variables with which they are concerned or with respect to the nature of the operations performed on those variables (25).

In Fig. 2 a schema is presented for illustrating the distribution of G_i activities over persons. Rows in this matrix represent group members and columns represent the T_i variables or conditions with which the group is concerned. A G_i entry indicates that a group member is responsible for, or typically engages in some sort of input activity with reference to a given T_i . A legend is provided which describes the type of G_i activity for

| | | Task Input Events | | | | |
|-------------|-------|-------------------|----------|----------|----------|----------|
| | | t_{i1} | t_{i2} | t_{i3} | t_{i4} | t_{i5} |
| Individuals | P_1 | g_{i1} | | g_{i3} | g_{i1} | |
| | P_2 | g_{i3} | g_{i1} | | g_{i2} | |
| | P_3 | g_{i3} | | | g_{i1} | |
| | P_4 | | g_{i4} | | | g_{i4} |

Legend:

g_{i1} --- scans periodically and reports
 g_{i2} --- observes on request and reports
 g_{i3} --- collates information
 g_{i4} --- records or stores information

FIG. 2. AN ILLUSTRATIVE MATRIX SHOWING DIVISION OF RESPONSIBILITIES FOR "GROUP INPUT" ACTIVITIES

which a given group member is responsible.

Considering only g_{i1} and g_{i2} entries in Fig. 2, it is possible to determine the extent of overlap or cross-checking in observations on T_i variables. For example, t_{i1} and t_{i4} have several alternative observer sources while other T_i variables have only one. If the distinctions among G_i activities are considered, the general function of any individual in the group may be surmised. P_4 , for example, acts primarily as a storage repository for information.

Of course the example presented in Fig. 2 is illustrative only. The particular G_i categories used may not be useful for tasks in general. More important, it may not be possible to characterize tasks generally in terms of particular allocations of G_i activities to persons. However, in most cases it appears that physical constraints or instructions make certain allocation patterns overwhelmingly probable.

Functional behavior. The most significant aspect of the functional behavior of G_i activities is the way in which these activities are actuated. For example, observations may be made according to a periodic scanning program; they may be triggered by relatively dramatic T_i events; or they may occur only upon solici-

tion by a group member who requires certain information. In general, studies in the literature do not evidence any systematic attention to the actuation of observations.

More notice has been given to the actuation of information reporting. Luce et al. (29), imposed time restrictions in a network study and Schacter (40), and others have controlled message transmission timing by having an administrator collect and distribute notes. The present writers have administratively manipulated actuation in an experimental comparison of "soliciting" versus "volunteering" instructions to subjects (26).

G_o Properties

Descriptive properties. G_o activities, comprising the "afferent" processes of the group, cover a wide range of behavior. Decisions and commands would be included in this class as would motor and verbal responses. For most group tasks of experimental interest, the required motor behaviors are comparatively simple or draw on previously overlearned skills. Examples of these are the operation of push buttons or switches (30, 39) or simple paper-cutting operations as in Schacter's study (40).

Spatial distribution. As in the case of G_i activities, specialization of G_o activities may occur through group agreement or administrative fiat. In the adaptation of Yerkes' multiple choice apparatus employed by McCurdy and Lambert (30), switches were distributed among subjects, while in the study of Schacter et al. on cohesiveness (40), there was also an explicit division of labor on the output side. Previously cited studies by the present writers have all entailed rather definite assignment of G_o activities. Other studies, such as the mechanical assembly task used by Carter et al. (10) enforce a division of

labor by physical restrictions. Some degree of specialization is required in tasks used by Haythorn (20) requiring a secretary or recorder, or by Moore et al. (34) in which a spokesman is appointed. Of course, studies focusing on role theory generally treat such specialization as the primary dependent variable.

As in the case of G_i activities, a matrix schema is useful for representing G_o distribution. Figure 3 presents such an illustrative matrix. In this case, G_o activities are conceived as having reference to certain T_o effects and, like G_i activities, they take various forms. In the example, P_1 is responsible for making most of the decisions which are then carried out by other group members. P_2 has the joint responsibilities for deciding appropriate action and performing it with respect to t_{o4} . The categories used in this matrix are, again, only illustrative.

Functional behavior. The temporal patterning and functional relationships of G_o activities constitute one of the most interesting aspects of the molecular task properties. Here, we shall emphasize those properties which best fit in with the over-all framework and with the primary focus on action-oriented groups.

As indicated in the illustrative example of Fig. 1, G_o activities may be described with respect to their dependence on G_i activities. That is, each type of response by various group members usually bears some relationships to certain classes of information received in the form of direct observations or of communications from other group members. Such "stimulus-response" associations may be specified by the task administrator (37), or they may be established by the group through trial and error processes as in the tasks used by McCurdy and Lambert (30),

| | | Task Output Events | | | | |
|-------------|-------|--------------------|----------|----------|----------|----------|
| | | t_{o1} | t_{o2} | t_{o3} | t_{o4} | t_{o5} |
| Individuals | P_1 | g_{o1} | g_{o1} | g_{o1} | | |
| | P_2 | g_{o1} | | | g_{o1} | |
| | P_3 | | g_{o2} | | g_{o2} | |
| | P_4 | g_{o1} | | g_{o2} | | g_{o1} |

Legend:

g_{o1} --- makes decisions as to adjustments

g_{o2} --- performs adjustments

g_{o3} --- standby adjustments

FIG. 3. AN ILLUSTRATIVE MATRIX OF DIVISION OF RESPONSIBILITY FOR "GROUP-OUTPUT" ACTIVITIES

Moore (34) and Perlmutter (35). The G_i - G_o relationships may be of a simple, one-to-one form or they may involve the dependence of each G_o element on many G_i elements (see Fig. 1). In some tasks, such as the ash tray cutting task used by Berkowitz (7), the G_o activities are functionally dependent only on the initial task instructions—there are no significant G_i activities. In general, it does not appear that the degree of systematic investigation of G_i - G_o relationships in group situations is at all commensurate with the importance of the problem or with the parallel interest in stimulus-response relationships in individual psychology.

Tasks may be distinguished not only by the form and complexity of G_i - G_o relationships, but also by the way in which these relationships are mediated by group communications. In the first place, there may be a distinct pattern or "network" of message transmission, or communication may occur on a diffuse, ad hoc, basis. If well defined networks exist, they may be described in terms of the physical facilities available, such as interphone systems or message cards, or by specifying more general topological features as Bavelas has proposed (6). A series of studies by various investigators (11, 28, 42) has employed networks described in this way as a primary variable.

| Individual as transmitter of information | Individual as Receiver of Information | | | |
|--|--|-------|-------|-------|
| | P_1 | P_2 | P_3 | P_4 |
| | P_1 | — | .55 | .20 |
| | P_2 | .35 | — | .70 |
| | P_3 | .50 | .20 | — |
| | P_4 | .50 | .60 | .30 |

FIG. 4. AN ILLUSTRATIVE "TRANSMITTANCE" MATRIX FOR A GROUP TASK

It is suggested that here again a simple matrix schema may serve adequately to describe the typical communication flow for a particular group task. In Fig. 4, a hypothetical "transmittance" matrix is shown in which each entry gives the percentage of information originally available to the persons identified by row headings which (within a given time) is transmitted to the persons identified by column headings. These units are of course arbitrary and could be replaced by other measures which proved more economical or meaningful.

Several advantages are suggested for a matrix representation as opposed to, say, a topological description. First, it is possible to represent gradations in communications between pairs of persons rather than limiting the description to yes-or-no terms. Further, if qualitatively distinct stages occur in the processing of information, as suggested by several investigators (3, 36) the communication channels obtaining for each phase are represented naturally by matrices, while summary properties may be inferred by conventional matrix procedures (38).

As a result of the foregoing dependent relationships and for inherent reasons, G_o activities will assume characteristic temporal distributions of occurrence. Inherent reasons might be the necessity for individual group members to concentrate on one activity at a time or the programming

of tasks in certain orders for reasons of efficiency.

The resultant, in any case, can be described in terms of the rate of G_o activities, in terms of their continuous or staccato occurrence, and according to whether they are homogeneous over time or tend to change in character as the task progresses.

T_o Properties

Descriptive aspects. The set of events T_o consists of all environmental conditions which are in any way affected or modified by group activities in G_o . In some tasks, such as the ordinary discussion task (4), there are no T_o events of importance. In other tasks, such as story reconstruction (33) or judgment tasks, there is little point in making a distinction between T_o events and their G_o counterparts. However, for any task in which the group is operating on an external physical environment, the description of T_o events forms an important part of task characterization.

The possible reinforcing or feedback properties of T_o events (as they influence T_i events) have been described above. An equally important function of T_o events is as criteria of the performance of group-task systems. In order to fulfill this function, the T_o events must have definite metric properties, and must be accessible for measurement and recording. Evaluation of a group-task system then entails "tapping off" these criterial events, e.g., the number of "enemy planes" shot down in an air defense task (25) or the accuracy of positioning in an assembly task (21).

Distribution. The loci of output effects for most laboratory tasks are coextensive with the corresponding G_o activities and hence of secondary importance. There are cases, how-

ever, where the relative positioning of T_o events may bear directly on the performance of a group-task system. An example is the construction task (8, 20), in which the possibility of making subassemblies may greatly influence the efficiency with which the group's manpower is utilized. In many military tasks, the fact that control outcomes are physically separated from each other (as in airplanes) is crucial to group organization and performance.

Functional behavior. The dependence of T_o events upon G_o activities may be characterized in several ways. First, there is the *determinacy* of G_o - T_o relationships—the degree to which physical consequences of certain behaviors follow mechanistically and without the intervention of chance factors. Beyond this, there is a question of the *contingency* of effects upon existing conditions. Obviously, if the T_o effects of a given response vary widely depending on the general state of the environment, group procedures for assessing relevant conditions and making careful decisions become increasingly critical. The construction of G_r - G_o relationships by the group referred to above is of course dependent on their recognition of these contingent effects. The recognition, in turn, is highly dependent on the degree of determinacy of G_o - T_o relationships.

An Extension to "Critical Demands"

It is believed that the foregoing framework provides a basis for a comprehensive and detailed description of any particular task. It might, for example, be used to organize a "blueprint" or set of specifications for the design of a laboratory task once the over-all purpose and broad features of the task was decided upon. The molecular properties suggested

by this framework are less successful however in conveying the meaning of a task in psychological terms or in affording a basis for comparison among tasks. The following sections attempt to meet these shortcomings of this—or any—molecular framework.

Consideration of tasks used for investigating individual characteristics readily shows that they are customarily grouped in terms of the types of behavior which they elicit or the skills or attributes that they tap. "Projective tests" such as the Rorschach, TAT, Blacky, and sentence completion forms, for example, have little in common except that they may be used to evoke a certain type of verbal behavior that is hypothesized to reveal various cognitive and personality traits. Similar assertions may be made with regard to intelligence tests, motor skill tests, or attitude tests.

By analogy, we may expect that the most useful method of classifying group tasks will be with reference to those aspects of group behavior or procedures which these tasks bring to the foreground. In other words, we would expect that the distinctive features of particular tasks will be the degree to which they require certain group behaviors for adequate performance. Such behavioral requirements will be referred to as "critical demands."

The postulation of constructs such as critical demands to bridge the gap between molecular task properties and social psychological variables poses two major problems: first, how to identify the critical demands; and, second, how to tie them back in with the molecular properties. It appears to us that both of these problems suggest a bootstrap process, in which initial hunches and investigations are

gradually supplemented by more systematic investigations.

Illustrative Critical Demands

As a possible approach to the initial identification of critical demands, we refer to an earlier report (38) in which three general functions in group-task performance were suggested. *Orientation* denotes the function of determining the condition of variables in the task environment or of determining the group's standing with respect to those variables. This is the "fact-finding" function, broadly speaking, and in support of this function a number of specific critical demands may be present. Examples would be individual or collective vigilance, requirements for collation or cross-checking of information, and storage or memory demands.

The second major function proposed was *mapping*, i.e., the process by which a group anticipates or learns the consequences of various action alternatives under various environmental conditions. Entailed in this are the demands for adequate feedback on response effects, demands for the transfer and application of previously learned environmental reactions to modified situations, and requirements for more complex storage of functional relationships between response classes and task outcomes.

The final major function was labelled *jurisdiction*, referring to the processes whereby response actions are chosen and decisions implemented. Subsidiary demands on the group relate to a satisfactory consensus as to objectives, a recognized chain of authority or rule of order, and, for many tasks, some fixed division of labor.

Other critical demands might be derived from these general functions, but the foregoing list should serve to

convey the nature of the suggested constructs. At this level of description, critical demands would afford intermediate conceptual guideposts between the molecular task properties and the more familiar concepts of psychology and social psychology.

Critical Demands and Molecular Task Properties

Although a thorough examination of the relationship between critical demands and molecular task properties is well beyond the scope of this report, several general observations may be in order. First, it will be noted that the orientation demands correspond roughly to T_i and G_i task properties; that mapping demands are related to G_r - G_o and G_o - T_o dependencies; and, that jurisdiction demands are most directly concerned with G_o properties. Within these broad groupings, however, it seems likely that each critical demand may stand in one-many relationship to several molecular properties. As an example, a high storage demand in orientation may arise from a combination of intermittent input and of stimulus material that is not readily coded in verbal form. Other combinations of various molecular properties may impose entirely equivalent storage demands. If this sort of one-many relationship holds in fact, the identification and validation of critical demands may be very difficult, but the need for such constructs becomes increasingly great.

A second point concerns the relationship between the demand level and the "difficulty level" attached to the associated molecular properties. Briefly, it would appear that critical task demands should be most prominent for intermediate difficulty levels rather than for trivial performance problems at one extreme or nearly impossible problems at the other. In

individual psychology, an intelligence test is not composed exclusively of items requiring the highest degree of cognitive ability. Similarly, a task designed to tap the mapping functions in groups should provide a reasonable degree of regularity in G_o - T_o relationships.

To summarize the preceding sections, it is suggested that, while a molecular description of task properties is useful, and even essential, it is hardly sufficient for purposes of designing and classifying tasks. An intermediate level of construct is represented by critical demands, which may be integrated about those combinations of task properties placing a high premium on certain facets of group performance. The illustrative set of critical demands here presented is based on a particular paradigm of group performance and should of course be revised or supplanted as a result of empirical and theoretical comparison of tasks.

An Application to a Representative Task

To test the usefulness of the task paradigm framework and the notion of critical demands, we shall examine a specific task, namely, the "common symbol" task used by Leavitt (28),

Christie et al. (13, 29), and, recently, Guetzgow and Simon (19). A large body of refined data have been obtained on this task under a variety of conditions. Our analysis will indicate that these conditions might be extended further, permitting much broader generalizations of the research results.

Briefly, the task is as follows: each of five S s is given a card on which are five out of six possible symbols. Only one symbol is common to all five cards. The subjects are to determine which of the symbols is the one they hold in common. Subjects are physically isolated by partitions, but each S may communicate with some or all other S s by passing message cards. The task is completed when all S s have obtained the correct answers.

Table 1 gives an abbreviated and schematic description of the task in terms of its molecular properties (as customarily administered). Perhaps because of the static nature of this task it does not appear very exciting when summarized in these terms. Of course this fails to do justice to the special purposes for which the task was designed, but it does raise the question as to whether those purposes might be expanded.

The critical demands imposed by

TABLE 1

| | Descriptive Properties | Spatial Distribution | Functional Behavior |
|-------|---|--|--|
| T_i | Easily distinguished symbols on cards | Uniformly distributed except for missing symbols | Independent of output present throughout task performance |
| G_i | Determine missing symbol and transmit to other station(s) | Uniform except that central persons may relay from distant persons | Reporting ad lib. or by request |
| G_o | Report on common symbol to other group member or experimenter | Essentially uniform | Dependent on G_i through network channels. No fixed phasing. |
| T_o | Experimenter corroboration of answer | Uniform | Occurs at termination of performance |

this task are predominantly in the area of orientation and, even there, are rather restricted. The principal demand on a group, as noted by Guetzgow and Simon (19) appears to be the need to set up an effective procedure for actuating and directing messages. Vigilance, in the customary sense, is not required, nor is any degree of storage required, since the relevant T_i variables are always present. The range of variables that must be scanned is limited to the six possible symbol types. The "mapping" process by which group members relate appropriate G_o activities to specific G_i activities (or, of course, T_i conditions) is a trivial one for this task and the jurisdiction of G_o activities similarly poses no great problem for groups.

Without attempting to be systematic or exhaustive, it is not difficult to suggest modifications of the task that might bring it closer to real-life situations. In the area of orientation, first, the symbols could be presented to individual group members successively (say by projections on a screen) so that storage of information and perhaps some individual vigilance was required. This would also have the effect of introducing new problems in the actuation of messages since stereotyped procedures would no longer be effective. Demands for coding and interpretation of information might be introduced if each input dimension comprised a class of symbols—such as all symbols having certain geometrical forms.

If the task were given the dynamic properties suggested in the above paragraph, real demands for mapping could also be introduced. As a simple example, it might be necessary for each group member to operate a certain switch, depending on which symbol was the common one, in order to step the display to a new pattern

of symbols with a new common symbol. Any very complex mapping demands, however, might very possibly compromise the real virtues of the original task.

The jurisdictional, or response executive, process could also be brought into focus by requiring differential responses to various common symbols. Assign a different person in the group to press a key for each symbol that was found to be held in common. By manipulating the frequencies with which various common symbols appeared, it seems likely that situations would arise in which the "optimal" communication procedures for a given net (as suggested by previous research) would be quite inefficient. This corresponds to organizational situations in which it is not feasible to funnel all information to either a fixed decision-node or to all group members.

The question may well be raised, "Why complicate the task? Isn't much of its appeal due to its clearness and simplicity?" The answer is that if, as we believe, this task does not now tap important group functions, a high premium would attach to any modifications which did more adequately measure those functions. If such modifications were introduced carefully and systematically, investigators could capitalize in various ways on the solid background of research data already obtained on the task. It appears that a program of extension of this sort can best be accomplished using a general task framework like that suggested in this report.

SUMMARY

We have undertaken in this report to attack the problems of adequately describing group tasks on a fairly wide front. First we have attempted to describe significant as-

pects of the behavior of a total group-task system by means of certain properties of the major classes of events hypothesized to occur in such systems. We have defended the inclusion of "modal" properties, describing typical behavior of persons or environmental variables, on the grounds that these properties characterize a task within limits, and that they may be partially controlled by apparatus design and by instructions. The suggested paradigm seems to permit a relatively comprehensive and realistic set of specifications for a task. On the other hand, it is recognized that a principal function of laboratory research on group tasks will be the evaluation of certain system components, represented in many cases by fairly abstract features or characteristics of groups, and that a large conceptual gap exists between the molecular group-task description and the primary referents of such research. For this reason, we have introduced the notion of "critical demands" which are based on broadly defined group functions but are amenable to more detailed specifications. Finally, an attempt was made to apply both the molecular and critical demand analyses to a widely used task and to show how the usefulness of this task could be further extended.

It is unnecessary to stress the tentative nature of this proposed ap-

proach to the problem of describing and classifying tasks, but it may be useful to point out immediate obstacles to a more enduring framework. First, the adoption of modal task properties is admittedly an interim measure. However, the translation of modal properties into objective properties cannot be more precise than our knowledge of the determinants of individual operator performance. Although tremendous strides have been taken in the analysis of certain individual responses in task situations, the responses studied often are not relevant to the analysis of individuals as components in group-task systems. A second limiting factor is the lack of an acceptable schema for denoting dimensions along which groups vary because of factors of composition or organization. Critical demands, in particular, can be identified only to the extent that we can identify unitary group characteristics. Most important, however, and possibly at the root of both the above limitations, has been the general neglect of task parameters in small group research. Real progress in the definitions of task properties will result from systematic studies of task variables, and increasing appreciation of the fact that hypotheses concerning relationships in small-group behavior cannot be generalized on the strength of haphazardly selected tasks.

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THE ROLE OF FRUSTRATIVE NONREWARD IN NONCONTINUOUS REWARD SITUATIONS¹

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This paper is based on the proposition that an adequate theory of instrumental behavior must involve three types of goal event: (a) *Rewarding events*—usually the presence of stimuli which evoke a consummatory reaction appropriate to some condition of deprivation; (b) *Punishing events*—noxious stimulation at the termination of a behavior sequence; and (c) *Frustrative events*—the absence of or delay of a rewarding event in a situation where it had been present previously.

Much attention has been given to the first two types of goal event. For example, in Hullian behavior theory (16, 18), to which this paper relates most particularly, both the onset of reward and the offset of punishment have been related to the more general reinforcement principle.

The third type of goal event, the frustrative event, was considered, in Hull's original statement (16), only as an occasion on which there is no reinforcement; nonreward of a previously rewarded response was not assigned any special role in the system. In a late article (19) and more particularly his last book (18) Hull did refer to the "frustration of an anticipation" and indicated—formally in Corollary 17—that the "abrupt ces-

sation of a customary reinforcement" will result, at first, in "a slight initial rise in $S^{\#}R$ due to emotion (D)" (18, Ch. 5). This is Hull's recognition of the motivational properties of non-reward. (In somewhat similar terms Skinner writes, "When we fail to reinforce a response that has previously been reinforced . . . we set up an emotional response—perhaps what is often meant by frustration" [40, p. 203].)

Basically, the position adopted here assumes that all learned, instrumental (goal-oriented) responses depend, to some extent, on classical-conditioned implicit responses. These responses are the learned counterparts of responses elicited by the three types of goal event—rewarding, punishing, and frustrative events—outlined earlier. This type of response, along with the response-produced stimulation associated with it, has been designated r_{G-SG} , the fractional anticipatory (antedating) goal reaction, and a construct of this sort has been employed extensively in connection with the development of positive excitatory tendencies (e.g., 1, 14, 15, 42). In the present terminology, r_{G-SG} is a general term covering all types of antedating conditioned response. Separately, these might be designated r_{R-SR} , fractional anticipatory reward; r_{P-SP} , fractional anticipatory punishment, usually termed fear or anxiety; and r_{F-SF} , fractional anticipatory frustration. The first two of these have had a considerable amount of theoretical and experimental attention; the last has not, and we will be concerned with it here.

¹ This paper is closest in form to a talk presented in May, 1957 to the Psychology Colloquium of the State University of Iowa. It includes material presented at a meeting of the Southern Society for Philosophy and Psychology in April, 1951; at a Psychology Colloquium at the Southern Illinois University in May, 1956; and in a Symposium at the meeting of the Midwestern Psychological Association in May, 1957.

Conceptually, anticipatory reactions, such as have been designated, are conditioned forms of goal or consummatory responses (secondary reinforcing reactions), which, through stimulus generalization and higher-order conditioning, come to be elicited by stimuli in an instrumental response sequence which antedate the goal. The fractional anticipatory frustration reaction may then be conceptualized as resulting from higher-order conditioning and generalization of secondary nonreward.

PURPOSE

This paper will develop the position (a) that under certain conditions nonreward is an active factor which may be termed *frustrative nonreward*; (b) that such frustrative events are antecedents to a primary, aversive, motivational condition, *frustration*; and (c) that a secondary (learned) form of this primary aversive condition, termed *fractional anticipatory frustration* (r_F-s_F), develops through classical conditioning and is the inhibitory mechanism in nonreward. The position will be taken that frustrative-nonreward events determine activating (drive) effects, which can be measured as an increase in the vigor of behavior which immediately follows the frustrative events, and are also responsible for inhibitory effects, which are at least partly responsible for decreases in strength of the instrumental behavior which is terminated by the frustrative event.

The reasoning in this position will be developed (a) by describing briefly experiments on motivational properties of frustration and indicating what seem to be their implications, and (b) by presenting a critical review of some experiments and an analysis and some preliminary data which seem to reflect, at least indirectly, upon the inhibitory properties of

frustrative nonreward, particularly in noncontinuous reward situations: discrimination learning and partial reinforcement situations.

SOME GENERAL COMMENTS

Definition of Frustration

We are in the position of using the term, frustration, according to a meaning restricted by the framework in which the term is used. It is a conceptualization of a hypothetical, implicit reaction elicited by nonreward after a number of prior rewards. Actually, we have been interested in certain active properties of nonreward following reward and no more than this is meant by the term frustration as we use it. We have been more interested in properties of nonreward, *per se*, and less interested in frustration in any of its perhaps richer, but certainly less precise, "emotional" and clinical meanings.

Compare our restricted usage of *frustration* with another relatively restricted one in essentially the same theoretical framework: in 1951, Brown and Farber (7) outlined an approach in which frustration was treated as "a hypothetical (defined) state or condition of an organism." The antecedents to frustration according to their view were various kinds of interference with an ongoing response: physical barriers, delay, the presence of an incompatible response tendency, work, or the omission or reduction of customary reward. The way we have used the term, frustration has an even more restricted meaning than that given by the definition of Brown and Farber. For the present, at least, we would designate by *frustration* a condition which is the result of the interaction of a single prepotent response tendency with events external to the organism; the term, *conflict*, would conceptualize that process which is

defined by the presence of two or more response tendencies of about equal strength which are incompatible; and a concept like Hull's I_R would be retained to represent work or refractory effects. It is doubtful whether any useful theoretical purpose can be served, at this stage, by conceptualizing several quite different manipulations with a single term even though the manipulations seem to produce similar effects. As a matter of fact, certain theoretical and practical difficulties are encountered when a distinction is not made between frustration and conflict. For example, one of the incompatible response tendencies in approach-avoidance competition may be frustration-motivated or, to be more exact, motivated by conditioned (anticipatory) frustration. Here Brown and Farber would have to distinguish two frustrations, one, defined by nonreward, partially determining a second, defined by competing tendencies. This difficulty will become clearer later in this presentation when we deal specifically with this case—the conflict of anticipatory frustration tendencies with positive or approach tendencies. The Brown and Farber treatment makes the situation ordinarily defining conflict an antecedent to frustration; ours, under certain circumstances, makes frustration one of the motivational antecedents to conflict.

"Theory" of Frustration

In a recent Nebraska symposium, Marx (25) refers to the approaches of Brown and Farber (7) and Amsel (3) as "two factor theories of frustration." The two factors which Marx identifies are frustration's contribution to generalized drive strength (D) and the presence of characteristic frustration drive stimuli (S_D). Actually ours is, if anything, a three-factor position; in any event, it seems

doubtful whether the word, theory, is appropriate here. The introduction of a motivational construct into a broader theoretical framework is not the statement of theory but merely the definition of a term; no new assumptions are involved. Once frustration is introduced as a motivational construct into the Hull-Spence system, it must be demonstrated to have the properties of other such terms in the system; and one is then committed to a given conceptualization of the term. Since we have also employed the construct, *fractional anticipatory frustration* (r_F-s_F), which has all of the properties of other fractional anticipatory responses, we likewise commit ourselves to classical conditioning principles in our use of that term. Frustration (F) is simply a motivational condition contributing to D and providing directive cues (S_D); and r_F-s_F is a classical-conditioned, antedating form of F .

MOTIVATIONAL PROPERTIES OF FRUSTRATION

There are many experiments which, directly or indirectly, seem to establish the motivational properties of nonreward. While some of the authors concerned have not offered a motivational interpretation of their findings, experiments by Miller and Stevenson (27), Finch (11), Rohrer (33), Sheffield (38), Stanley (43), Marzocco (26), Lambert and Solomon (23), and Holder, Marx, Holder, and Collier (13) could be taken as supporting a motivational interpretation of frustrative nonreward. In the present discussion, however, emphasis will be placed on a few experiments by the writer and his colleagues, and on a confirmation of certain aspects of these experiments by Wagner (46). The development of an hypothesis concerning inconsistent reward ef-

fects, which will be offered, depends pretty heavily on what these experiments seem to demonstrate. In fact, the experiments which follow in this section and the next were a case of one thing leading to another, and an attempt will be made to indicate the reasoning which ties them together. The experiments discussed relate only to the D properties of frustration and not the S_D properties, since the latter type of experimentation, in which we have also been engaged to some extent (6, 31) is not so relevant to the present discussion.

In all of the experiments of this section the situation is essentially the same: two instrumental (running) responses are elicited in series, the apparatus consisting of a start box, a first runway (Runway 1), a first goal box (G1), a second runway (Runway 2) and a second goal box (G2) in serial arrangement. The apparatus was designed to conform to our definition of a frustrating event, and it permits the distinction between the frustrated response (running in Runway 1) and the frustration-motivated response (running in Runway 2). The major dependent variable in these experiments is running time (or speed) in Runway 2; the independent variables are manipulations of reward and nonreward in G1, and also certain variations of the stimulus properties of Runway 1.

The question asked in the original experiment was essentially this: Does nonreward following consistent reward have any special motivational properties? Does it invigorate responses which follow it?

In an experiment by Amsel and Roussel (5), 18 male albino rats were trained under hunger motivation to run down Runway 1 into G1 where they found food, then leave G1 and run down the second runway into G2 where they found food again. Their

time to traverse the alley between G1 and G2 was measured during a preliminary period and, when running time had stabilized, a series of test trials were run, on half of which S was not rewarded in G1 prior to running in Runway 2. The results indicated that after the Runway-2 time had stabilized during the preliminary period, nonreward of the Runway-1 response was followed by shorter Runway-2 times (higher speeds) than those following reward of the Runway-1 response. This difference between the vigor of performance following reward as compared with nonreward has been termed the *frustration effect (FE)*.

Our next experimental question is: Must a period of continuous (100%) reward in G1 necessarily precede the demonstration of *FE* or will *FE* appear when, from the outset of training, a response is partially reinforced as in the test trials of the Amsel and Roussel experiment. An unpublished experiment by Roussel (34) first demonstrated the *development of FE* when the Runway-1 response was rewarded 50% of the time from the beginning of training. It indicated that after about 20 trials of 50% reward of the Runway-1 response, the curves showing running vigor following reward and nonreward in G1 begin to separate and remain separate. The implication seems to be that whatever develops during continuous reward in G1 to produce, with subsequent nonreward, a relatively *immediate FE*, also develops during 50% reward in G1. There is, in the 50%-reward (partial-reinforcement-of-Runway-1-response) condition, a gradual development of the frustrative properties of nonreward, as inferred from the performance in Runway 2.

This leads to a third question: What is developing, presumably as a

function of the number of rewarded trials, which makes nonreward frustrating? The guess was r_R-S_R . This can be tested by varying those factors which, by definition, would affect the strength of r_R . If factors which are, by definition, favorable to r_R development are introduced in Runway 1, the development of r_R , and therefore F , should be enhanced.

A recently published experiment by Amsel and Hancock (4) tested this hypothesized relationship between r_R factors and F . Two experiments were performed, in each of which the similarity between Runway 1 and G1 was the variable defining differential strength of r_R in Runway 1 and upon entry to G1. The two experiments differed only in that in the first—which was like the Amsel and Roussel study—a continuous reward period in G1 preceded any non-rewards, while in the second—which was like the Roussel experiment—there was 50% reward and 50% non-reward in G1 from the start of training. The results confirmed those of the previous experiments with the important additional finding that the magnitude of FE was greater in each case when the conditions for r_R arousal were better.

The results of these two experiments support the hypothesized relationship between r_R and strength of FE . Another unpublished experiment by Amsel, Ernhart, and Galbrecht confirms the Amsel and Hancock results and provides further support of the r_R hypothesis of frustration by showing a greater FE when Runway-1 length was 8 feet than when it was 2 feet; presumably, this provides a longer period of time for the elicitation of r_R and hence a greater probability of its being present at sufficient strength when S runs into the frustrating situation.

Finally, there is a follow-up study by Wagner (46) which confirms aspects of all of the Tulane studies just described and adds a very important control not included in those studies. Using what is essentially the Amsel and Roussel two-runway situation, this experiment demonstrates again: (a) the immediate frustration effect following 76 preliminary rewards in G1, and (b) the developing nature of the FE when the Runway-1 response is partially reinforced (50% reward in G1) from the outset. A small separation of the curves showing running speed in Runway 2 following reward and nonreward in G1 occurs on the 29–36 trial block and continues quite large and significant thereafter.

The added control in the Wagner experiment is a group which is never rewarded in G1. It is particularly important because of a criticism which has been directed at the frustration interpretation of the Tulane experiments. The criticism is that faster running in Runway 2 following non-reward may be due not to increased D from frustration, but simply to the fact that nonreward (not giving a drop of water or a bit of food) does not temporarily decrease D , as it does on reward trials. Wagner's control condition, a group never rewarded in G1, shows, if anything, slower Runway-2 running than the following-reward performance of the other groups. In the Amsel and Roussel paper, it was pointed out, in considering this objection, that one might expect weaker performance in such a control condition because of the absence of "prefeeding" or "predrinking" effects—the invigoration of performance which follows the giving of very small amounts of the incentive in advance of performance rewarded by such incentives (8, 28).

ANTICIPATORY FRUSTRATION IN NONCONTINUOUS REWARD SITUATIONS

The reasoning of this section depends heavily on the idea that, in partial or noncontinuous reward situations, F is a consequence of nonreward only after several rewarded trials have occurred and have permitted the development of r_R ; and that, before this, nonreward trials are not frustrating. The previous section presented substantial support for this generalization.

Actually, there are two types of situation in which a response pattern which occurs relatively consistently is rewarded inconsistently: (a) the partial-reinforcement situation, and (b) the early stages of discrimination learning. The thesis to be defended is that in such inconsistent reward situations frustrative factors are involved; that in discrimination learning they actually facilitate the stimulus selective process; and that in the partial-reinforcement situation they account for the greater resistance to extinction observed following non-consistent reward acquisition. We will need now to work with two frustration concepts, one in addition to the primary motivational factor which our experiments seem to demonstrate. This second frustration concept is the classical-conditioned, anticipatory form of F which can be identified as operating during an instrumental sequence after that instrumental sequence has been non-rewarded as well as rewarded some number of times. This factor we have termed *fractional anticipatory frustration* (r_F-s_F).

Before proceeding, the status of this r_F factor should be clarified. In 1951, at a meeting of the Southern Society for Philosophy and Psychology, this construct was introduced in

a paper entitled, "A Three-Factor Theory of Inhibition: An Addition to Hull's Two-Factor Theory" (3).² The burden of this paper was the notion that while the Hullian theory of inhibition was an "empty-goal-box" theory, many of the experimental findings with which we had to deal required inhibitory concepts related to nonreward. To quote the earlier discussion:

Our point of departure is the fact that there is, in the Hullian system, no postulated relationship between inhibition and goal events.

According to the Hullian position, inhibition develops initially because responses occur and recur, and for this reason only. So long as responses are reinforced, this inhibition due to responding (*reactive inhibition*) is, for the most part, offset by increments due to learning. Early in training when the learning increments are large, the inhibitory effects are more than offset; later in training when the increments are smaller, and especially under conditions of massed practice, these inhibitory effects due to responding may not be sufficiently offset; so that even consistently rewarded behavior may begin to grow weaker. This work factor is the primary inhibitory concept for Hull. His second inhibitory concept, *conditioned inhibition*, is the learned factor in inhibition, and it is postulated as beginning to develop at a stage where accumulation of reactive (work) inhibition is sufficient to produce some decremental effect upon behavior.

Hull's two-factor theory of inhibition has been successful in explaining many phenomena of response decrement, particularly in dealing with classical conditioning and human motor learning (experimental extinction, spontaneous recovery and incomplete recovery, disinhibition, reminiscence, etc.). Where the theory suffers is in attempting to account for certain aspects of response decrement in what might be called goal-oriented

² Seward (36) has mentioned, but not elaborated, a similar anticipatory frustration factor (r_{SF}) at about the time of the writer's original paper. One difference between the two treatments, apart from detail, was that while Seward suggested the factor as part of a somewhat radical departure from the Hullian approach, it was not considered by the writer to be inconsistent with the general Hullian type of theorizing.

learning situations, e.g., instrumental training, trial-and-error learning, and discrimination learning. It is the writer's opinion that in this latter kind of learning situation omission of goal events as determiners of some part of response decrement overlooks an important inhibitory factor, *fractional anticipatory frustration* (r_F-s_F) (3).

It was pointed out that while primary frustration, a motivational condition, could not be regarded as inhibiting the instrumental response which preceded it, anticipatory or conditioned frustration could be regarded as having inhibitory properties since it could move backward from the goal region to affect weakening of an instrumental response.

This introduces no new principles beyond the definition of frustration as a primary, aversive motivational condition—the ascribing of certain active properties to frustrative nonreward. It would follow, from the conceptualization of F as an aversive condition, that the stimulus component (s_F) of the fractional anticipatory frustration reaction would tend already to be connected to reaction potentials—or to become conditioned to reaction potentials—antagonistic to the instrumental approach tendencies in question. If F is an aversive condition, s_F should be associated with avoidant response tendencies which, in our situation, would compete with locomotion toward the previously rewarding goal region. The r_F-s_F concept depends only on the demonstration of the motivational properties of nonreward following reward. Beyond this only established classical conditioning principles are involved.

Now, back to inconsistent reward situations and the role of r_F-s_F . Figure 1 shows a conception of the role of frustrative factors in discrimination learning and in partial reinforcement, employing a type of diagram used by Spence which points up the im-

portance of classical conditioned factors in instrumental behavior.

Partial reinforcement and discrimination learning procedures are highly similar; in fact, they are almost identical if we compare partial-reinforcement training to the early stages of discrimination training with separate (successive) rather than joint (simultaneous) presentation of stimuli. In both, at the outset, S is rewarded on some occasions and not on others for the same instrumental response. The difference is that, in partial-reinforcement experiments, E is training S to make the same response on every trial, whereas in discrimination learning different stimulation is involved when S is rewarded and not rewarded, and S comes ultimately to respond (or not respond) selectively; but only, as a rule, after S has learned to respond nonselectively on the basis of partial reinforcement. The role of partial-reinforcement factors in discrimination learning is developed in a study of Wilcoxon (48).

The sequence of events as we conceptualize it, outlined in a study by Wilson, Weiss, and Amsel (49), is as follows:

1. The development of r_R-s_R (r_q-s_q in Fig. 1) with early rewards; nonreward is ineffective at this stage;
2. With the development of r_R-s_R , nonrewards elicit more and more frustration;
3. When nonrewards elicit frustration, the instrumental sequence cues previously evoking r_R now also evoke r_F , and these antedating goal responses are temporarily in competition. (This should produce temporarily increased variability in the instrumental response; also temporarily decreased average vigor);
4. Since r_R and r_F cannot be elicited separately by differential cues in partial reinforcement, as they can in the latter stages of discrimination

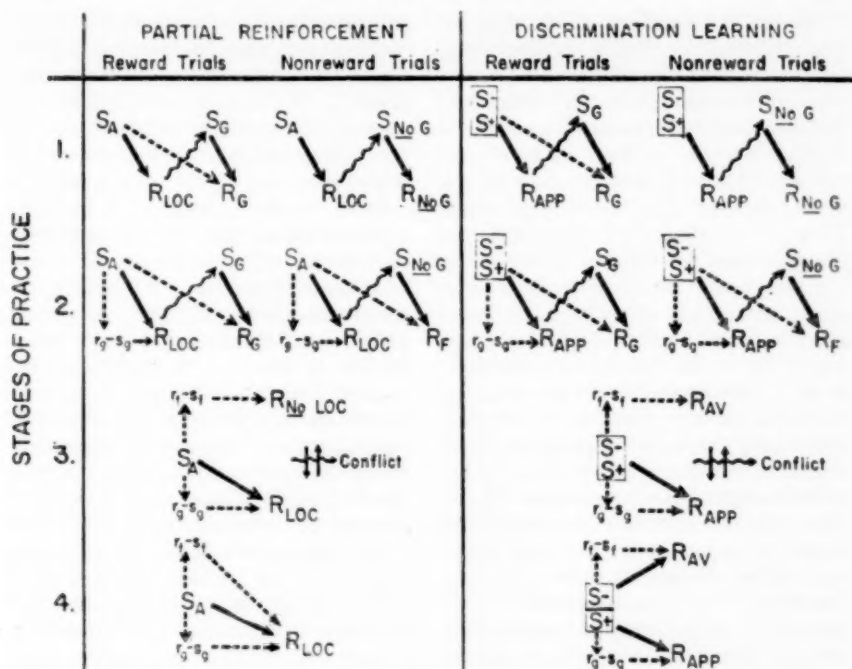


FIG. 1. DIAGRAMMATIC REPRESENTATION OF THE HYPOTHESIZED ROLE OF FRUSTRATIVE FACTORS IN PARTIAL REINFORCEMENT ACQUISITION AND IN DISCRIMINATION LEARNING.

learning, and since partial-reinforcement training is such that running to the goal box is reinforced more than avoiding it is— S is removed from the situation at the end of the trial if it runs to the goal box, reducing conflict and frustration as well as the relevant need on rewarded trials— s_F becomes associated with the instrumental approach response in the latter stages of partial-reinforcement training.

5. When S s are switched to extinction, the partially reinforced S s have been trained to respond (approach) in the presence of antedating frustration stimuli, whereas the consistently reinforced S s have not.³

³ After the final preparation of this paper an article appeared by Kendler and others (22) in which an interpretation of the partial reinforcement effect is offered which seems

In the next sections, arguments and evidence are presented to support the suggested explanatory role of frustrative factors in inconsistent reward situations.

Frustrative Factors in Discrimination Learning

In this section, the main argument will be that inhibitory effects in discrimination learning cannot be accounted for adequately in terms of work factors, directly or indirectly.

almost identical to the explanation of Wilson, Weiss, and Amsel (49) which is elaborated here. Although the main difference between the interpretations lies in the preference by Kendler et al. for a "neutral" designation of the nonreward effect rather than a frustrative designation, the authors apparently fail to recognize the essential similarity of their interpretation to the earlier one of Wilson, Weiss, and Amsel.

Under the heading of discrimination learning we deal with situations in which two or more stimuli are originally equivalent in their response-evoking properties and, ordinarily, learning is said to have occurred when a response is made to one of the stimuli and not the other(s). This type of learning situation has been termed *stimulus selection* (17).

The stimuli (discriminanda) in discrimination learning may be presented either separately and successively (as in the Pavlovian differentiation experiments) or jointly (as, for example, in the majority of studies employing the Lashley jumping technique). In either case, *the response involved is not a differential factor*: in the separate presentation situation the response occurs at first to both stimuli; in the choice situation, neither discriminandum is consistently in one place, so that a correct response (e.g., jump to the left) on one trial may be an incorrect one on the next, insofar as responses may be differentiated according to spatial factors. It is, therefore, difficult to see how inhibition based on responding (work) could develop differentially in discrimination learning. If, in terms of Hullian theory, I_R were not differential, its effects should not become more strongly conditioned to the negative stimulus ($S^{-I}R$) than to the positive stimulus ($S^{+I}R$). In his last treatment of discrimination learning Hull states that the "reduction in reaction potential [$S^{-E}R$] is due in the main to the accumulation of conditioned inhibition" (18, Ch. 3). Presumably, Hull is referring to reduction in the net reaction potential $S^{-E}R$ which results from the subtraction of $S^{-I}R$ from $S^{-E}R$. In any event, there would seem no basis for deducing greater conditioned inhibition to S^{-} than to S^{+} .

To emphasize this point, let us con-

sider specifically the case of discrimination learning (differentiation) with separate presentation of discriminanda. If, in a series of trials, the presentations of S^{+} and S^{-} are randomly ordered, with S^{+} always reinforced and S^{-} never reinforced, the subject responds originally to both discriminanda, but learns eventually to respond to S^{+} and not to S^{-} . Inhibition of response to S^{-} which develops cannot be attributed to a state which depends upon the prior presence of I_R since, in the early discrimination training, the same response was made to both discriminanda. In fact, one of the frequently noted early indications of discrimination learning which precedes any change in correctness of choice is that the response to S^{-} becomes less vigorous (longer latency, lesser amplitude) than that to S^{+} . There is nothing in the Hullian system which suggests that I_R is in any way related to reinforcement or nonreinforcement; consequently, if $S^{I}R$ is to be employed in the explanation of discrimination learning, one would have to hold that it develops to both S^{+} and S^{-} , but that in the case of S^{+} it is offset by the incremental effects of the reinforcements ($S^{H}R$). The difficulty of this latter position is encountered when $S^{+H}R$ approaches a maximum. At this point $S^{I}R$ should begin to show its effect upon the response to the positive discriminandum and soon the discrimination should again be lost—unless one assumes that $S^{H}R$ and $S^{I}R$ are nonpermanent states which are not merely incompatible but actually weaken one another.

An alternative theoretical course is to assume that, in discrimination learning, work is not a *differential* factor, but that the positive and negative goal events determine positive and negative excitatory tendencies. (Nissen [30] in his approach-avoid-

ance interpretation of discrimination learning has argued convincingly for essentially this position.) If we assume that on nonreinforced trials there is present, in the goal region, a frustration reaction, it is possible to identify the negative excitatory factor conditioned to $S-$: since the negative discriminandum is present immediately preceding the nonreinforcement, its trace becomes conditioned to the frustration reaction ($S-H_{RF}$). This anticipatory (anteceding) frustration response would then be evoked by $S-$, and the response-produced stimulus (s_F) would become connected to not responding. Actually this last connection (s_F-H_{noR}) is, presumably, strongly present in adult organisms through previous experience; or, at least, some response is already connected to s_F which determines that organism's characteristic reactions to anticipated frustration.

An experiment by Eninger (10) could readily be interpreted in terms of our present position. Rats were run in a modified T maze, in which the arms and goal boxes could be made either black or white. Four conditions were run: In Condition 1, the Ss learned to choose the white alley to find food in a white goal box and not to choose the black, which led to nonreward in a black goal box. Condition 2 was the same as 1 except black led to food in a black goal box, whereas white led to no food in a white goal box. In Condition 3, the white alley led to food in a black goal box and the black alley to food in a white goal box; and Condition 4 was the reverse of 3. Thus, as Eninger writes: for Groups 1 and 2, the approach properties acquired by the reward box brightnesses could generalize to the positive cue alley, and the avoidance properties acquired by the non-reward box brightnesses could generalize to the negative cue alleys. For Groups 3 and 4, however, the approach properties would

generalize to the negative cue alleys, while the avoidance properties could generalize to the positive cue alleys. (10)

The results clearly confirmed the expectation: Groups 1 and 2 combined took less than one-half the number of trials to reach the criterion of learning than did the combined Groups 3 and 4. Clearly these results lend themselves to interpretation in terms of r_{R-S_R} and r_{F-S_F} , where s_R is conditioned to approach responses and s_F to avoidance.⁴ The findings of a more recent experiment by Grice and Goldman (12) agree with those of Eninger with the additional suggestion that the generalization of extinction to the negative choice-point cue is a more effective factor in discrimination than generalization from the reward goal box to the positive choice-point cue.

In view of the definition of frustration that we proposed—as a state resulting when a *consistently* reinforced reaction is not reinforced—one might question the assumption that frustration would be aroused on nonreward trials in discrimination learning. We will consider this question in relation to two cases: (a) Where there have been several reinforcements of $S+$ before differential reinforcement of $S+$ and $S-$ begin. An example of this is an experiment by Antoinetti cited by Hull (18, p. 63). In this case, there has been 100% reinforcement of $S+H_R$ before the first nonreinforced ($S-$) trial is run. Here the conditions for frustration are clearly present. (b) The more frequently encountered case in discrimination learning, especially with joint presentation of stimuli, is the one where there is differential reinforcement of

⁴ This is in effect what Eninger assumes: "... whenever a neutral stimulus is repeatedly experienced in close association with frustrative non-reward, the stimulus acquires the capacity to elicit avoidance behavior" (10, p. 398).

$S+$ and $S-$ from the outset, apart from any preliminary training trials in which S s are rewarded for approaching the goal region. In this case, responses are reinforced only 50% of the time from the start, if the subject responds randomly or falls into a position habit during the early training trials. The question is: Does frustration develop under these circumstances? The experiments by Roussel, by Amsel and Hancock, and by Wagner, previously cited, seem to indicate that nonreinforcement early in partial (50%) reinforcement training is not followed by a frustration effect, but that the frustration effect (an increase in vigor of behavior immediately following nonreward) develops on nonreward trials and begins to be apparent after several trials of partial-reinforcement training. This suggests the interpretation that r_{R-S_R} develops first during partial reinforcement training or during the first stage of discrimination learning, which is not different from partial reinforcement so long as the subject is responding nondifferentially to the discriminanda. The Amsel and Hancock data provide further support of this position by demonstrating that the frustration effect is greater and more prolonged when conditions for eliciting r_{R-S_R} are most clearly present in the situation. Then, only after $S^H r_{R-S_R}$ is sufficiently strong (where S represents the undifferentiated stimulating aspects of the situation leading to approach behavior), does nonreinforcement lead to frustration, and it is from this point that discrimination develops. And this development of discrimination depends upon the building up of several differential habit connections: (a) the traces of $S+$ and $S-$ are conditioned to the positive and negative goal reactions to produce $S+^H r_{R-S_R}$ and

$S-^H r_{F-S_F}$; and (b) s_R and s_F become associated with differential responses (approach and avoidance; responding and not responding; more or less rapid responding; etc.).

In 1936, Spence (41) provided a theory of discrimination learning deduced from conditioning principles. According to this theory, discrimination learning proceeds by the building up of the excitatory tendency of $S+$ through successive reinforcements, and the building up of the inhibitory tendency of $S-$ through successive nonreinforcements. On the basis of these assumptions and certain others, namely, (a) that excitatory and inhibitory tendencies generalize to other stimuli along some continuum, and (b) that the tendency $S+^H R$ is stronger than the tendency $S-^H noR$, Spence has successfully accounted for certain transposition phenomena in $S-R$ terms. According to the present analysis, r_{R-S_R} and r_{F-S_F} are the mechanisms behind the positive and negative tendencies that Spence's theory demands. Unlike the present Hullian position, Spence's earlier theory assigns an active property to nonreinforcement in the development of the negative tendency.

Frustrative Factors in Partial Reinforcement

Attempts to account for the relative fixation of responses acquired under partial-reinforcement schedules walk a well-beaten path. The explanations have been many and varied. Beginning with the "common sense" expectancy interpretation of Humphreys (e.g., 21), there followed the "response-unit" explanation offered originally by Skinner (39) and tested by Mowrer and Jones (29); the Hull-Sheffield interpretation (37) in which traces of previous goal events and

stimulus generalization decrement are the important factors; the discrimination or sensory integration hypothesis of Bitterman and Tyler and others (9, 44, 45); Weinstock's contiguity hypothesis (47) which suggests that partial reinforcement provides an opportunity for the habituation of competing responses on the nonreward trials during acquisition; some recent Pavlovian interpretations of Razran (32), the most convincing of which makes partial reinforcement "a case of the efficacy of repeated post-extinction reconditionings"; a more recent interpretation of Logan, Beier, and Kincaid (24) in terms of r_G factors; and a still more recent, contiguity-type interpretation of Hulse and Stanley (20) which suggests that partial reinforcement training increases resistance to extinction because the partially reinforced *Ss* have acquired during acquisition something to do in the goal box on nonreward trials.

What we are calling partial reinforcement should be differentiated from the Skinnerian periodic reinforcement situation or fixed-ratio situation. We deal here with discrete-trial learning of a highly-spaced character. It is very likely that quite different principles and explanations may be required in the two cases, since the chained type of responding involves consideration of effects of stimulus traces from previous responses and previous goal events, whereas the discrete, highly-spaced-trial situation, as we see it, involves primarily consideration of certain developing associative tendencies. Spence (42, p. 123) makes this point very well when he compares the Skinnerian partial-reinforcement situation to training a rat to take many steps in a runway for terminal reinforcement. It should be apparent

that the "response-unit" type of explanation is only applicable to the chained, highly-massed type of situation.

Some evidence for the present interpretation of the partial reinforcement effect (*PRE*) comes indirectly from experiments relating *PRE* to secondary reinforcement factors:

The Amsel and Hancock experiment demonstrated that the *FE* was greater when better secondary reinforcing stimuli (r_R -eliciting stimuli) were present in Runway 1. With partial reinforcement of the Runway-1 response, the *FE* developed more strongly when the stimuli in Runway 1 were like those in *G1* than when they were not. According to the present position, this means that under partial reinforcement conditions the relative presence or absence of secondary reinforcing cues on the nonreward trials of acquisition should enhance or reduce the *PRE* in extinction. The reasoning is as follows: When, on nonreward trials, secondary reinforcing cues are minimal, there is less r_R evoked, therefore less *F*, therefore less r_F . This results in less conflict during acquisition and ultimately less connection of s_F to the rewarded response; therefore, less persistence of responding in extinction.

In connection with the present interpretation, experiments by Rubin (35), Hulse and Stanley (20), and Tyler (44) are interesting. Rubin found that when secondary reinforcement was minimized during partial reinforcement acquisition (generalization factors were also reduced from acquisition to extinction), continuous reinforcement groups were more resistant to extinction than partial groups. Hulse and Stanley report a reliable *PRE* when secondary reinforcement was present on every trial during acquisition, but not when sec-

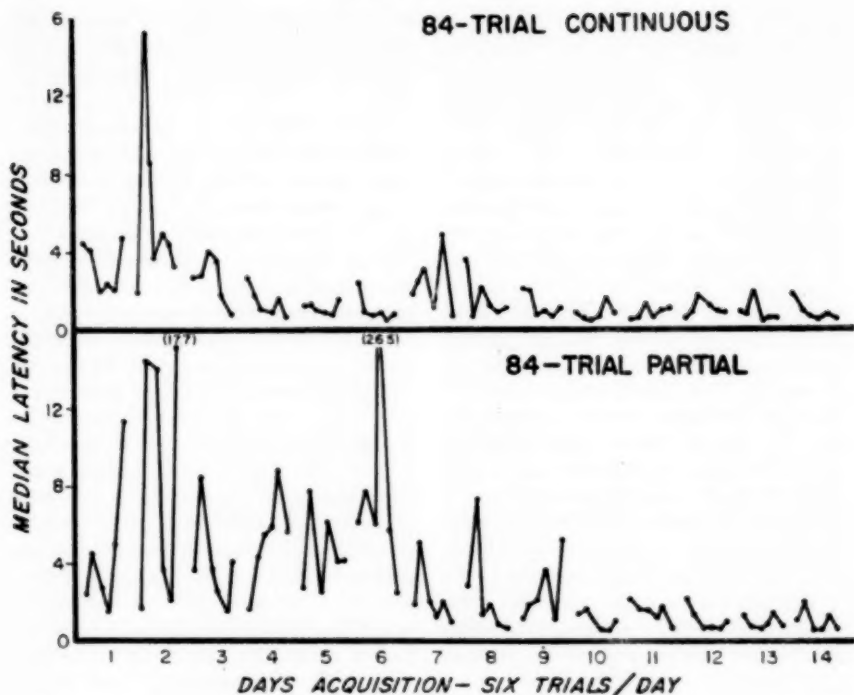


FIG. 2. ACQUISITION CURVES SHOWING COMPARISON OF THE VARIABILITY PATTERN IN THE 84-TRIAL GROUPS FOR LATENCY (STARTING TIME) MEASURE.

ondary reinforcement was omitted on the nonrewarded training trials. Tyler ran three acquisition conditions: *Consistent*—100% reinforcement with same color goal box on every trial in acquisition and extinction; *Same*—50% reinforcement with same color goal box on every acquisition and extinction trial; and *Reversed*—50% acquisition with, for example, black goal box on reward and white goal box on nonreward trials of acquisition followed by extinction in the black goal box, i.e., the goal box in which *S* was rewarded during acquisition. While the 100% group extinguished more quickly than either of the 50% groups, there is some indication that the Same group was more resistant to

extinction than the Reversed group.

In all cases, not having the stimulus which accompanies reward present when *S* is nonrewarded in partial reinforcement acquisition seems to reduce the partial reinforcement effect.

There are two obvious major implications of our frustration interpretation of partial reinforcement learning and extinction. These are:

1. That the *PRE* will be evident only after some critical number of training trials, i.e., only after *s_p* becomes conditioned to running; the number of trials will depend on the training situation.

2. That partial reinforcement acquisition will be more variable than continuous reinforcement acquisition

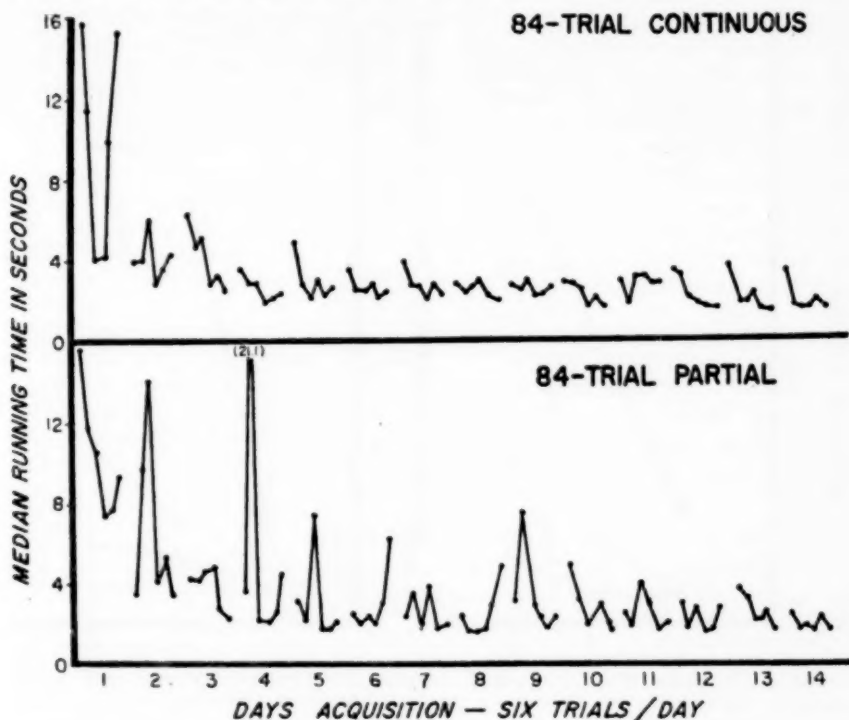


FIG. 3. ACQUISITION CURVES SHOWING COMPARISON OF VARIABILITY PATTERN IN THE 84-TRIAL GROUPS FOR THE RUNNING TIME MEASURE.

performance in the early-middle (conflict) stages of training, when r_R and r_F are in competition.

Some data have been collected in an attempt to test these implications and a preliminary experiment related to them will be described. This study involved a small number of *Ss*, 20, who were divided into four acquisition groups differentiated by two variables: (a) partial (50%) versus continuous (100%) reinforcement, and (b) 24 versus 84 acquisition trials. The four groups can be identified as 24P, 24C, 84P, and 84C. The apparatus was an enclosed, straight, eight-foot runway, and measurements were made of (a) time from raising of the

start-box door to reaching a point six inches into the alley (latency), and (b) time to traverse the middle, seven-foot portion of the runway (running time). After the designated number of acquisition trials, 18 extinction trials were run. During both the acquisition and extinction phases, there were 6 trials per day with a minimum of 10 minutes between trials. The results are shown in Figs. 2, 3, and 4. Figures 2 and 3 show acquisition latency and running time respectively; Fig. 3 shows latency and running time in extinction.

As Figs. 2 and 3 indicate, the continuous and partial groups start at about the same level on Day 1. Dif-

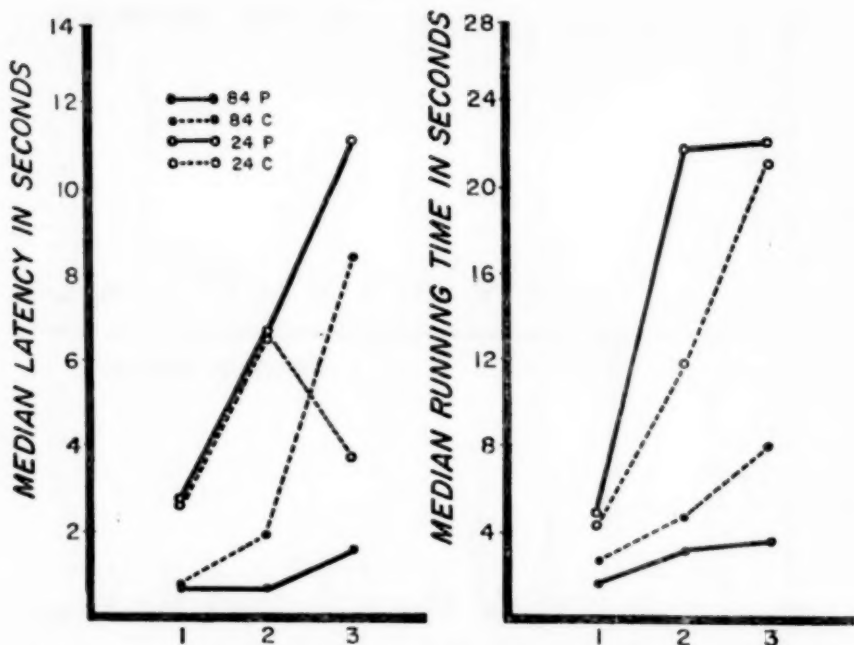


FIG. 4. EXTINCTION CURVES SHOWING COMPARISON OF RESPONSE PERSISTENCE IN THE 84-TRIAL AND THE 24-TRIAL GROUPS FOR THE LATENCY AND THE RUNNING TIME MEASURE.

ferences in variability develop, becoming most noticeable at about Day 4 and continuing through about Day 9. It is interesting that the stage of development of increased variability in these data corresponds roughly to the stages in the Roussel, the Amsel and Hancock, and the Wagner data where the *FE* begins to appear with partial reinforcement of the Runway-1 response. It should be noted that, on the last 5 days of acquisition, the 84P data look about like those for 84C—there seems to be little difference here in either absolute level of performance or variability, as on Day 1. Comparing Figs. 2 and 3, there seems to be greater relative variability differences between 84P and 84C in the latency measure; however, for both measures, the difference in vari-

ability between the 84P and 84C curve points was significant beyond the .01 level for Days 4 through 9 (F is 22.28 for latency, 8.02 for running time), but not significant at the .05 level for the Day 1–3 block, or for the Day 10–14 block.

The extinction data (Fig. 4) show that after 24 acquisition trials, the continuous group is slightly (but not significantly) more resistant to extinction than the partial group; but after 84 acquisition trials, the partial group is significantly more resistant to extinction than the continuous group, the magnitude of the difference increasing with successive extinction days.

As a rough indicant of "emotionality" (2), a record was kept of urination in the experimental apparatus.

None of the 24-trial Ss urinated in the apparatus either during training or extinction. This can be taken tentatively to mean that conflict had not yet developed in the training. Of the 84-trial Ss, three of the five partials urinated 18 times between the 49th and 84th acquisition trials; and two of the five continuous Ss urinated eight times between the 61st and 84th acquisition trials. Nine of the ten 84-trial Ss urinated during extinction.

The latency and running time data and the urination data are interpreted as indicating the development, in the partial reinforcement situation, of F and r_F-s_F , with resultant con-

flict between the thirst-motivated running response and incompatible anticipatory frustration-motivated responses. According to this position, the greater variability of running time and starting time in the 84-partial group is regarded as evidence of this conflict. The decrease in variability of starting and running times on about the last five acquisition days in the 84P group is taken to indicate that s_F has become conditioned to running. The appearance of a PRE after 84 acquisition trials, but not after 24 such trials, is also consistent with the present hypothesis regarding the relation between PRE and frustrative nonreward factors.

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Formerly, journals that could not be delivered because subscribers had not notified the APA of a new address were reclaimed by the APA, and the journal was remailed to the subscriber at his new address. This was always expensive. Recent changes in the postal laws and regulations have made the expense prohibitive. Undeliverable copies are now destroyed by the Post Office. Subscribers who do not receive a journal because of an address change are charged the regular single issue price for a replacement copy.

So—when you move—

Notify the postmaster at your old address and guarantee that you will pay the forwarding postage.

Notify the APA Subscription Office as early as possible—by at least the tenth of the month preceding the month when the change should take effect.

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